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THE PSYCHOLOGICAL REVIEW

THE NATURE OF THE EFFECTIVE STIMULUS IN ANIMAL DISCRIMINATION LEARNING: TRANSPOSITION RECONSIDERED

DONALD A. RILEY

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When an animal learns to discriminate between two stimuli lying on the same physical dimension, what is the nature of the effective stimulus with which the response is associated?

In 1937, Spence (13) presented a theory which assumed that the animal learns to approach the absolute properties of the positive stimulus and that independent of this, it learns not to approach the absolute properties of the negative stimulus. In those situations in which the animal learns to discriminate between two points on the same physical dimension, Spence defined the effective stimulus in conventional physical units—for example, the number of square centimeters in a size discrimination, or the number of foot candles in a brightness discrimination. Spence contrasted this absolute definition of the effective stimulus with Köhler's earlier assumption (7) that the animal responds to the relationship between the two stimuli, and learns to go to the larger or the brighter of the two stimuli. Spence made some additional assumptions that enabled him to predict the transposition of the learned response to pairs of test stimuli which differ from the training stimuli. In contrast to the relational theory, which made no quantitative predictions, Spence's theory predicted that transposition would decrease as the test stimuli became less and less similar to the training stimuli. This prediction has been confirmed in several experiments (1, 6, 9, 13).

This paper will present evidence that Spence's assumption that the animal responds to the absolute properties of the stimulus, defined in units such as absolute size or luminance, is untenable. This evidence also casts doubt on Spence's explanation of the decline in transposition. Two alternative reinterpretations of the facts of transposition will then be considered. Before presenting the new evidence, Spence's theory will be described, together with an example of the type of evidence supporting it.

Near and far transposition tests. The meaning of the terms "near transposition test" and "far transposition test" can be given schematically. Let letters A through J represent points on a physical continuum, such that the ratio of B to A is the same as C to B, and so on. If the *S* is originally trained to choose B in the presence of A and B, then a test with B and C is a near transposition test. A test with any other pair of adjacent stimuli (e.g., H and I) is a

far transposition test. For example, if in original training the ratio of the magnitude of the positive to the negative stimulus is 2/1, then in the near transposition test, the ratio will be 4/2. In the far transposition test, the ratio might be 64/32, or 128/64. Although Spence and others have discussed other combinations of stimuli, in the interest of simplicity and brevity, the discussion here will be restricted to the case in which the test stimuli are of greater magnitude than the training stimuli.

The sample experiment which will now be described in this simplified form is one by Kendler (6). Two stimuli serving as cues to the presence or absence of reward were presented to the *S*s (in this case, rats). These stimuli differed only in that one had a photometric value of .011 foot candle while the other had a value of .63 foot candle. Going to one of the stimuli (e.g., the brighter) was always followed by reward. Going to the other was followed by nonreward. After learning, the animal was tested with other pairs of stimuli. Different animals were tested at different intensities, but in each test the ratio between the two stimuli was the same as in original training. In each test, the animals preferred the more intense stimulus, but the magnitude of the preference decreased as the intensity of the test pairs increased.[1] That is, far transposition is weaker than near transposition. Similar results have been found with size as the critical dimension (1, 9, 13).

The absolute theory. Because Spence's theory is given in detail in many references, it will be summarized here only in very brief form. It assumes that if the animal is regularly rewarded when it approaches one point on a stimulus dimension, and is regularly nonrewarded when it approaches another point on the same dimension, the animal will develop corresponding approach (Excitatory) and nonapproach (Inhibitory) tendencies. These E and I tendencies will be elicited by other points on the dimension, but the intensity of the elicited tendency will decline as the distance of the test point from the training point increases. The shapes of the E and I gradients are positively decelerated, and, in the simplest case, have the same equation. The tendency to make an approach response to any point on the dimension will be a simple function of the difference between the two opposing tendencies. Since, at points above the training stimuli on the stimulus continuum, the I generalization gradient will fall more rapidly than the E gradient, transposition will occur. As both curves fall, the greater E-minus-I value of the more intense stimulus becomes relatively smaller, and less transposition occurs.

Limitations in the generality of the absolute theory. While it is true that this absolute theory does account for transposition, and for the drop in transposition in the far test, some recent research, especially that of Lawrence and his associates, appears to present findings incompatible with Spence's account of transposition. Baker and Lawrence (2) found transposition with rats following conventional training on a size-discrimination problem, but did not find transposition when the training stimuli were presented successively. Unfortunately, the two conditions differed in one respect other than simultaneity vs. successiveness. The animals in the simultaneous condition learned to criterion, and thus received almost 100% reinforcement toward the end of training. In contrast, the animals in the successive condition had the same num-

[1] The amount of transposition in Kendler's experiment did not decline monotonically when the test stimuli were more intense than the training stimuli. The animals transposed more to the most remote stimuli than to those of intermediate value. The reason for this is not clear, but one possibility is that the voltage changes that were introduced changed the wave length of the light. This in turn might influence brightness discrimination.

ber of trials with the negative stimulus as with the positive stimulus throughout training, and consequently were reinforced only 50% of the time. Baker and Lawrence pointed out that in spite of this methodological flaw, the results are not consistent with Spence's theory.

Another apparent contradiction of the absolute theory is an experiment by Lawrence and DeRivera (10). Animals were trained in a discrimination problem in which, on each trial, two doors showed the same stimulus pattern. The bottom half of each door was always the same medium gray (shade No. 4 in a set of seven shades). The top half of each was either a shade of gray three steps lighter (No. 1) than the bottom half, or a shade of gray three steps darker (No. 7) than the bottom. The animals were trained to jump to one door when the upper parts were lighter than the lower, and to jump to the other door when the upper parts were darker. After training, the animals were given combinations that tested between a relational interpretation of learning and an absolute interpretation. For example, with No. 3 over No. 1, the animal should jump to the side associated with the lighter gray, according to the absolute hypothesis, because both cards are lighter than No. 4; but since the relationship is dark over light, they should jump the other way according to the relational hypothesis. The results predominantly supported the relational hypothesis.

Clearly, the results of Lawrence *et al.* appear to be in contradiction to Spence's theory that the animal responds to the cues in an independent fashion. As a possible solution of these apparent contradictions, Lawrence and DeRivera (10) and Hilgard (5) have suggested that under some circumstances the animal responds relationally, and that under other circumstances it responds absolutely. That is, perhaps it responds absolutely in a simple simultaneous discrimination situation, but not in others. It is possible, however, that *the decline in transposition in the far test* has not yet been adequately explained, and that an understanding of *this* phenomenon will resolve the apparent inconsistencies. Then, we will be in a better position to state testable alternatives concerning the nature of transposition.

A contrast interpretation of the drop in transposition. If, instead of defining each stimulus merely in terms of the patch alone, it is defined as a specific relationship between the value of the patch and the value of the surround, then the decline in transposition might result merely from the change in the relationship being responded to. Such a proposal is, of course, not new. In human perception, the effect of contrast on apparent brightness has been known for many years. Recently, Wallach (15) has shown that two test stimuli of radically different illumination will be judged approximately equal in brightness if the ratio of the test-stimulus intensity to surrounding intensity is the same for both stimuli. Similarly, Gibson (3) has shown that perceptual size constancy in humans depends on such factors as the texture gradient of the stimulus field surrounding the test stimulus. It is not unreasonable to assume that apes and rats might behave in a similar way.

An experimental test of the contrast hypothesis. The specific hypothesis to be tested is that after a brightness discrimination has been learned, there will be a drop in transposition on the far test only if the ratios of the test stimuli to their surround are changed.

The experiment was similar to Kendler's in that the rats were trained to discriminate brightness. All animals were trained in a modified Lashley jumping apparatus to jump to the more intense of two stimuli. The doors and the surrounding stimulation were illuminated from projectors hung near the top of the room. The different illuminations on the doors and the surround

were determined solely by slides in the projectors, and by the size of openings in iris diaphragms on the projectors. During training, the brightnesses of the two doors and the surround, as measured by a Zeiss photometer, were in the ratio of 10/3/1, the surround having the value of 1. The surround had a reflected illumination of .05 foot candle.[2] During training, the rats were run 10 trials a day by the noncorrection method. Forced trials were used to break position habits. Following training to a criterion of 19/20 correct responses, all animals were tested under four different conditions. Each animal was tested on only one condition in a day, and on days between tests the animal was retrained to a criterion of 9/10 correct responses. Each test consisted of ten trials during which the animal was rewarded regardless of the correctness of its choice. The order of conditions was counterbalanced. The four conditions were: (*a*) near absolute—the two test patches were transposed one step up, and the surrounding stimulation was the same as in original training; (*b*) far absolute—the two test patches were made 5 steps brighter and the surrounding stimulation was the same as in original training; (*c*) near ratio—the same as (*a*) except that the illumination of the surround was increased so that the ratio of intensities of the test patches to the surround was the same as in original training; and (*d*) far ratio—the same as (*b*) except that the illumination of the surround was increased so that the ratio of the intensities of the test patches to the surround was the same as in original training. Sixteen hooded rats of the Long-Evans strain were used.

Predictions. For this experiment to offer a test between the absolute and the contrast hypotheses, it is essential that there be significantly less transposition on the far absolute condition than on the near absolute condition. If this requirement is met, then the absolute theory predicts a similar drop from the near ratio to the far ratio condition, for the only two parts of the stimulus situation that have been differentially reinforced are being changed, just as they are in the absolute conditions. On the other hand, the contrast hypothesis predicts a drop in transposition from the near to the far absolute condition but no such drop in the ratio condition. If the ratio present in training is present in the test, then the animal should transpose, regardless of the absolute values of the stimuli.

TABLE 1

MEAN NUMBER OF RESPONSES TO THE BRIGHTER STIMULUS IN TEN CHOICES

	Near Absolute	Far Absolute	Near Ratio	Far Ratio
Mean	9.25	6.94	9.19	9.69
SD	1.09	1.84	1.36	.54

Results. The main results are shown in Table 1. Their meaning is unambiguous. The animals transpose almost perfectly in all except the far absolute condition. There were no effects of order. The only effect is a highly significant drop in transposition under the far absolute condition. A t test was run in which for each S the difference between near and far ratio was subtracted from the difference between near and far absolute. The t of 4.87 is significant at the .0002 level of confidence, for 15 degrees of freedom.

Interpretation of the results. It is clear, at least in the case of brightness discrimination, that the animal responds to the relationship between the test stimulus and the background. It is a change in this relationship rather than a change in the absolute value of the stimulus that causes the breakdown in transposition. While this experiment

[2] I wish to thank J. Gulberg, Department of Zoology, University of California, for supervising the measurement of the physical stimuli, and Arnold Mechanic, who participated in the running of the experiment.

does not provide an explanation of transposition per se, it does show that the drop in transposition in the far test is not critical evidence for Spence's explanation of transposition. The most immediate effect of this finding is to force a reconsideration of possible interpretations of transposition. Two such possible interpretations that seem to this writer to be reasonable alternatives will be considered. Again, for simplicity, the discussion will use brightness discrimination as the example.

A reformulation of the E and I gradients hypothesis. If the effective stimulus is redefined as the ratio of the training or test patch to the surround, then it is possible to maintain the assumption that the animal responds separately to the two training stimuli. In this case, the appropriate description of the stimulus intensity dimension will also be expressed in such ratios. When an animal has been trained to approach one stimulus (defined in ratio terms) and not to approach the other, E and I tendencies will be reduced to the degree that test ratios change from training ratios. If these redefined generalization gradients are of the form postulated by Spence, this hypothesis can then account for transposition and for the decline in transposition with changing stimulus ratios.

Can this interpretation account for the findings of the other experiments that were critical of Spence's absolute hypothesis? Lawrence and DeRivera's experiment is somewhat complex for the present analysis, because relationships within the stimulus patch were changed in the tests, but the surrounding stimulation was always the same as in training. Nevertheless, their results at least do not appear inconsistent with this hypothesis. If the two parts of the stimulus patch are responded to in the way that the stimulus surround was responded to in the present experiment, then their animals learned to jump left when the ratio on the cards was in one direction and right when it was in the other direction. As the ratios were changed in the test conditions, the tendency to make the relational response decreased.

On the other hand, this reformulation of the absolute theory does not account for the failure of Baker and Lawrence to find transposition following successive training. Their experiment therefore seems critical, and should be repeated with the same pattern of reinforcements in the successive and simultaneous groups.

A reformulation of the relational hypothesis. Since the decline in transposition in the far test cannot be considered as evidence for an absolute definition of the stimulus, the question of the adequacy of Köhler's original relational hypothesis is reopened. In his discussion of transposition, Köhler merely described the animal as having learned to go to the larger or lighter part of the stimulus. This restricted theory is still inadequate, for the reason pointed out by Spence. It cannot account for the decline in transposition in the far test. Nor does it help Köhler's explanation to say that the figure-ground relationships determine the phenomenal brightness of the two test stimuli, and that the animal approaches the stimulus which is phenomenally brighter, for this still would not predict a breakdown in transposition in the far test. Another unworkable relational assumption is that the animal responds to the ratio between the two figure-ground ratios. This hypothesis would state that in the present experiment the animal responds to the relationship of 10/1 (the brighter) to 3/1 (the dimmer)—that is, to a relationship of 10/3 or 3.33. Now in the critical far-absolute test, the values are about 4,100/1 for the brighter stimulus and 1,230/1 for the dimmer. This of course reduces to 3.33 and perfect transposition is predicted. Since transposition is known to decline under these circumstances, this hypothesis is inadequate.

It seems clear that a workable relational hypothesis will be one that predicts perfect transposition as long as all stimulus values in the critical dimension are multiplied by the same factor. If the values in the original training are in the ratio of 10/3/1, then any test situation in which the ratio is the same must give perfect transposition.[8] To the extent that any of the three parts of the stimulus deviates from this, transposition will decline. One possible interpretation of such behavior would be that the animal approaches a stimulus that is defined by two intensity ratios, and does not approach one that is defined by two other intensity ratios. That is, the animal learns to approach a training stimulus which has an intensity that is a certain multiple of the intensity of the surround and a certain multiple of the intensity of the other training stimulus. Similarly, the animal learns not to approach a stimulus that has an intensity that is a certain multiple of the intensity of the surround and a certain fraction of the intensity of the other training stimulus. This hypothesis has no trouble in accounting for a decline in transposition in the far-absolute situation of the present experiment. It is not so clear why there is no drop in transposition in the near-absolute situation, for here there is also a change in the ratios. One *ad hoc* interpretation is that, under the usual conditions of the transposition experiment, the near-absolute test is not a discriminably different relationship from the relationship present during training. There are a variety of reasons why this might be so. For example, since the difference between the training stimuli and the surround is substantial, a change of only one step may be relatively hard to detect. Further, twenty-four hours intervene between the last training trial and the first test trial. This increases the difficulty of discriminating between the two relationships.

This particular relational hypothesis is very similar to the reformulated absolute hypothesis. It differs only in that each critical stimulus is defined by two brightness gradients rather than just one. A direct test between these two hypotheses can be made in an experiment in which the stimulus area surrounding each stimulus patch may be independently varied. Then, following training identical to the present experiment, the ratio of test stimulus to surround could be kept constant, as in the ratio conditions of the present experiment, but the relationship between the two test stimuli could be changed from that of the training situation; e.g., the stimulus-surround value of the positive stimulus might be kept at 10/1, while the negative is changed from a training value of 3/1 to a test value of 30/10. As long as the stimulus-surround ratios are constant, the reformulated absolute hypothesis should predict that the animal would continue to approach the test stimulus that has the same ratio as the positive training stimulus, even though the relationship between the test stimuli is changed. This should be true regardless of the amount or direction of the change. The proposed relational hypothesis would predict breakdown in transposition as the ratio of the test stimuli to each other becomes more different from that of the training stimuli, even though the stimulus-surround ratios are the same as in original training.

There may be other relational hypotheses that would be consistent with the present facts. For example, Teas and Bitterman (14) have described conditions under which the rat appears to respond to the stimulus as a total configuration rather than responding selectively to different components of the

[8] Although Köhler explicitly made this point when discussing the general problem of transposition (cf. 8, p. 218), he did not apply this principle either in the design or in the analysis of specific experiments. In discussing the experiments, only the changes in the test patches were considered.

stimulus. The only difficulty in applying this hypothesis to the present data is that this experiment was run under conditions that are probably closer to the condition in their study that elicited selective reactions from the rat.

Two further implications of the present experiment. It is worth noting that the present study has implications not only for transposition, but for the closely related problem of stimulus generalization along quantitative continua. It appears that there is a real possibility that if the relationships existing in original conditioning are present in generalization tests, stimulus generalization will be complete regardless of absolute stimulus values. Directly related to this point is an experiment by Perkins (12) that indicates that stimulus intensity dynamism is probably a contrast phenomenon.

The second point is more of a question than a direct implication of the present experiment. How much of transposition might be accounted for by subcortical or even retinal mediation rather than by cortical processes? Some time ago, Hebb (4) presented evidence that brightness discrimination is probably controlled subcortically. More recently, Motokowa (11) has presented evidence from retinal recordings of the frog indicating that action potentials are related to brightness contrast rather than intensity.

SUMMARY

Evidence has been presented that in animal discrimination learning, as in human perception, the effective stimulus to which the animal responds must be described as a relationship between parts of a stimulus complex. This finding allows a reinterpretation of Spence's explanation of the decline in transposition when test stimuli are remote from training stimuli. It also sets the conditions for a reinterpretation of transposition. Two alternatives are discussed.

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THE DOMAIN OF HOMEOSTASIS[1]

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In the halls of our neighboring physiology department hangs the celebrated motto of Claude Bernard: *Le fixité du milieu interieur est la condition de la vie libre.* Of late, in our own discipline, as well as in physiology, one may observe a spreading and unchallenged faith in the virtue of this apothegm and its sequel, that all the "vital mechanisms" are devoted to the maintenance of this stability. It is thought to contain the germ which, cultivated by Cannon (3) into the full-flowered doctrine of homeostasis, now offers general shelter for psychologists. The "maintenance of equilibrium" has often been taken as the unifying principle of "motivation," and more recently as fundamental to all action of the living (1, 5).[2] As a model for behavior the principle of homeostasis presents itself in rivalry with the time-honored formula of stimulus-response or of any more modern equivalent.

Clearly this is a possibility of such importance that the meaning and domain of "homeostasis" should be given careful thought. Homeostasis and adaptation are sometimes treated as though one implied the other. But definitions of the two terms are usually not thus restricted, and the facts do not justify an inference of this sort. *The Wisdom of the Body* in both title and content shows that Cannon was strongly impressed with the "adaptiveness" of homeostatic regulation, and he showed no qualms about including purposive clauses in his sentences. He observed, nevertheless, that ". . . at times effects are produced which apparently have little or no value to the organism," and that infants and lower animals lack many homeostatic mechanisms. (Though he did not discuss the point, it is doubtful that he meant these were ill adapted.) The meaning of "adaptation" is none too clear in a scientific context, and even if it were established that a certain event is adaptive, it would follow only with an unknown probability that it is also homeostatic.

Two definitions of homeostasis seem possible on the basis of current and historical usage. It is difficult to say which is more legitimate, for they were doubtless commingled in the original writings. Cannon, and in the earlier period Bernard, were evidently led to the general conception because of some physiological facts which came to light. These discoveries showed that certain physiological variables are more constant than one would expect in a changing world, and that there are some specific mechanisms, now called negative feedbacks, which act, when a change occurs, in such a manner as to minimize the change. Either the first or the second part of the preceding sentence may be taken as the basic meaning of homeosta-

[1] Based on a lecture given at McGill University and University of Oslo, September and October, 1956. Assistance in preparation was given by Contract Nonr 908-03, Office of Naval Research.

[2] The form of Bernard's principle implies that he did not think of it as universal; the "free life" seems to be something beyond the regulation he is describing. It is very clear that Cannon (3) did not think homeostasis ruled everything, for he asks ". . . freedom for what?" and answers "In summary, then, we find the organism liberated for its more complicated and socially important tasks because it lives in a fluid matrix, which is automatically kept in a constant condition." Generally he regards the action of skeletal muscles as disturbing to homeostasis.

sis.[8] In other words, homeostasis can mean, and evidently does sometimes mean, simply the existence of a certain constancy in the face of change. On the other hand, one may use the name only for a certain type of mechanism. In the one case it is an end, in the other a means. (Of course, both meanings at once may be intended, though such restriction seems unusual.)

Though often not distinguished, the two meanings are not the same. Stability, resistance to change, or even restoration of a prior state is not invariably a sign of a negative feedback. This could, no doubt, be demonstrated by a mathematical argument, but the point can be made as convincingly with less formality.

Evidently the behaving organism is one of those physical structures called "systems." It qualifies as one of these because it has outputs—movements of body, hands, vocal cords, etc.—in some kind of relation to inputs, some of which are usually called stimuli and some of which are more general conditions. If we follow the usual presumption, this relation results from the operation of the same physical laws which apply to other systems. Accepting this identification, whatever we may know of systems—from everyday experience as well as from the mathematically developed theory of systems—will come to our aid.

Although one could not hope to decide exactly what happens on such general grounds, the field of possibilities can be helpfully delimited. If, for example, the principle of conservation of energy places certain restrictions on a system, it would be an extraordinarily bold hypothesis which would propose that the organism transcends them.

In the case at hand—i.e., the organism's stability—we may look at simpler systems to find, if we can, the same sort of stability. To begin with a very humble example, let us reflect on the behavior of an ordinary screen door being pushed by a small boy who likes to experiment. This, too, is a system with an input. If he unhooks the spring the boy will be pleased to find that the more push he gives the farther the door will go before it stops. It will be even more of a delight to find, after the spring is attached, that the door now pushes back at him. The more he pushes, the more the door reacts, and when he lets go the door will return to its original place. In more technical words, he has now observed that the door has a nonlinear response to a step-function input, and returns to zero after a square-wave input. It is displaced less than expected by an input force, and has "automatic" stabilization at a point of equilibrium. By other examples one could show that such a system can oscillate (as even a screen door can bang twice if conditions are right). These kinds of behavior are in fact to be found in any system which contains a reacting element—such an element as produces a restoring force as a funtion of input force, and this is a very common feature of systems. Restoring forces are provided by springs or other elastic bodies, condensers and inductors, heat-absorbing elements and chemical processes. Some elements of this kind are present in all systems, and it is a major engineering problem to construct a system in which their effect is too small to matter. With a moment's thought we can find all of these elements, except inductors, in the living organism. There is elasticity in

[8] Cannon wrote ". . . constancy is in itself evidence that agencies are acting, or ready to act to maintain this constancy." This implication would cover other mechanisms than feedback, and indeed he considers "storage" of simple capacitative sort an example of such an agency. In considering specific constancies, however, Cannon apparently takes it for granted that the "agency" is a feedback, even though the mechanism is unknown (e.g., 3, Ch. 8).

all the tissues, and in muscles and blood vessels it is known to be important; electrical and heat capacities exist throughout the body, and the piling up of chemical energy occurs in many places.

Clearly then, the "stability" of a response system is not much of an argument for the existence of a feedback mechanism. The same effects, to the closest approximation we are likely to be able to measure, can be brought about in a simpler fashion with a system which is reactive. To complete the indirect argument one would need to exclude in some way the possibility of the simpler mechanism in the case at hand; still better evidence would come from a direct demonstration of negative feedback.

At this point it is interesting to note a statistical argument which leads to the same conclusion.[4] Suppose a system can be in a certain number of states, ordered in some respect from lowest to highest. For simplicity, then suppose that each of these states is equally probable and in no way contingent upon any other. Upon finding the system in a state toward the top of the scale one would predict that its next state, if there is one, will be a lower one, simply because there are more states below than above it in the scale. Their summed probabilities will be greater. Certainly if the system is already at the top of the scale, its next state will always be lower, because by definition there is nowhere to go but down, and the best prediction would be the mean of the possible states. If the organism is already in its middle state one would, of course, predict its next state as a departure from that, but the mean of the possible states would still be the best prediction. (If this is not to be counted as a "next state," the best prediction would be as close as possible to the mean.) Further, suppose that a measured property, such as blood pressure, is the resultant of a considerable number of factors acting according to this description. The measured variable then tends actually to assume the mean position, merely by the random action of the many factors. An additional property of the model, in either simple or multiple form, is that the probability of finding it in a state deviating from the mean by a certain value decreases with the size of the deviation. (These consequences will also follow from assumptions more general than the ones used for illustration.)

It is hard to believe that the organism is so disjointed as these postulates would make it. Yet one may wonder whether the laws connecting states of the organism are so many that the effect is the same after all.

Inasmuch as stability and equilibration are so common in systems, the term *homeostasis* would diffuse over the landscape if it were used in a functional sense to specify a certain result. If it settles evenly on everything and distinguishes nothing, then to say that a system is homeostatic is tautological, though possibly useful.

According to its second meaning, homeostasis would be defined as a particular sort of process, regardless of result. Identified as a negative feedback its basic requirement is that some energy is taken from a later part of the system and introduced into an earlier part in such a way as to oppose the change produced there by the original input. Certainly quite a number of such mechanisms in the organism are known; there are others sometimes alleged in rather easy-going interpretation, for which one might desire better credentials. Physiologists (as well as psychologists) seem sometimes to start their interpretations with the postulate that every process is homeostatic.

[4] This line of thought was given me by C. J. Burke.

If homeostasis as a process is to serve as a general model for behavior, we should be able to say what information there needs to be to decide that a system is homeostatic. From the definition it follows that three things are necessary: (*a*) We should know what the system is, at least to the extent of identifying its input and output. (*b*) We should be able to demonstrate that energy ("signal") is, or at least can be, carried in a reverse direction. (*c*) We should know that this energy has a negative sign with respect to the original process when it re-enters the main path of the system. If these three things are the meaning of homeostasis in the narrower sense, it is plain that we need a clear idea of the first and a reasonable presumption of the other two, lest an assertion of homeostasis be but empty words. One should be on guard against theorizing which starts from one of the three requirements and draws upon nothing but a rich vocabulary for the other two.

We may contrast two examples of supposed homeostatic mechanisms, one which seems complete and another which seems filled out *ad hoc*. First, in the case of the stretch or myotatic reflex, we know the system concerned. Its output can be taken as the length of the muscle. The feedback is initiated by the receptors in the muscle and is carried backward by the afferents. Its effect is negative with respect to muscle lengthening. In the well known regulation of the heart the case is much less clear. The known fact is that receptors in the large arteries are stimulated by blood pressure increments and initiate a reflex which slows the heart rate. Then, to fill out the homeostatic picture, it is said that this reduces blood pressure. In this instance we collide with some troublesome facts. By Starling's law, an increased rate would simply reduce the stroke volume and have just about no effect on pressure in the arteries. And according to Marey's law, another empirical classic, a slow pulse is associated with high rather than low pressure. To add to the confusion, there are receptors in large veins, responsive to raised pressure there, which *increase* the heart rate by the so-called Bainbridge reflex, and it has been asserted that this too is homeostatic: it keeps down the stroke volume of the heart. At this point it seems we might conclude that the beautiful simplicity of homeostatic regulation has been too appealing, even in physiology.

In this instance the trouble seems to be an uncertainty about what is being regulated by a known mechanism. Of course, so long as a process has any effect at all, it is always possible to find something or invent something which it regulates, and in each case one would have quite a wide choice. This is a particular danger when homeostasis is taken as a general model. We may be offered variables or entities which exist only to be opposed by homeostasis, and conversely homeostatic processes which are said to exist only because they are opposite to some known effect, according to a sort of Hegelian logic.

An extreme example of such speculation, once offered in all seriousness, runs as follows: People have a homeostatic mechanism for maintaining social esteem, or the awareness of it. So, when they become aware that they have fallen in the opinion of their fellow men, they take action to raise their standing until their awareness of esteem reaches the point of equilibrium. The factual basis of this, so far as visible, is that people sometimes do things that others approve of, and it is doubtful that we know any more about this after hearing the theory.

A process entitled to the name of homeostasis by meeting the three criteria would not necessarily return the organism to its undisturbed state. Being a

negative feedback it would tend to lessen departure from such a condition, but need not be so powerful as to prevent it. The milder sort of feedback would tend to "overshooting" and oscillation, while admitting some change. Quite possibly this sort of feedback is the more usual in bodily processes.

Where we do have homeostasis, in the sense of a negative feedback, there are implications which would be important to a general theory. One of these deserves the emphasis which may be given it by a special name: *heterostasis*. The following example shows what the word is intended to mean. In humans and some other animals body temperature is said, quite correctly, to be subject to homeostatic regulation. A large part of the regulation on the high side, for animals like us, is accomplished by the evaporation of water secreted through the skin. A rise in external temperature, or an increase in heat production, is followed by a slight rise in internal temperature which activates the sweating mechanism through a certain well known route. And as perspiration is evaporated, heat is removed from the body and temperature lowered. It seems pretty clear that the regulated output of the system is temperature. But while this is being regulated, what is happening to the sweating mechanism? Necessarily every bit of reduced variation in temperature is bought by increased variation in sweating. Likewise where temperature is regulated during cold, there is, on the other side of the picture, an increase in heat production. This, one may call *heterostasis*.

On an argument from the conservation of energy, the case of sweating is not a special one, but rather is typical. In general, the organism, let us say, is acted upon by a force of a certain energy. The displacement produced by that force cannot be neutralized without the use of further force, inasmuch as the energy of the new state cannot be made to vanish. In the case of homeostasis, it is the organism itself which provides the restoring energy, by increased sweating or shivering, by changes in circulation, changes in digestion, and ultimately changes in fuel consumption. In cases of changed energy relations between the organism and environment, then, homeostasis can be maintained for one variable only at the expense of heterostasis in at least one other.

The chief homeostatic systems, like that for temperature regulation, seem to have feedback circuits with rather short loops, turning back from a point near the output and opposing the system response at some point not much earlier. Yet there are some feedbacks, perhaps longer ones, which regulate the admission of energy to the organism. Such are the tympanic and iridic reflexes, and possibly certain mechanisms which affect the sensitivity of afferent nerves. By operating at the input these stabilize the whole system to some degree, and heterostasis is not so obvious. Nevertheless one must recognize that it exists. Because the system operates as an amplifier, introducing energy along the way, it is less costly to oppose system response at an early stage. Yet even here a certain amount of work must be performed: the tympanic and iridic muscles do, of course, consume energy which they would not if they were not regulating input. Their action, therefore, shows greater variation while the main auditory and visual channels are showing less.

The point seems general that an organism's constancy in one aspect implies variability in another. It is speaking rather loosely, therefore, to say that an organism as a whole maintains an internal constancy in spite of external variations. Rather, some features of its internal state are kept constant by homeostasis, some by simpler means, and some vary in compensatory fashion.

There is one final argument for the

generality of homeostasis to be weighed: the argument from its necessity for survival. If survival were absolute, the argument would be more compelling, for it would be difficult to see how anything but an *ad hoc* mechanism could counter all the vagaries of the environment. But organisms and species are both mortal, and it seems quite possible that their limited survival can occur without their having the best of imaginable mechanisms for its assurance. It is hard to say just what life span would be permitted by the expected combination of good and bad accidents.

So far the argument has concerned cases in which there is a tendency toward equilibrium somewhere. But there are physiological mechanisms which work toward the opposite end, lowering the threshold for an activity which is already under way. There is, for example, the facilitation effect described by Gellhorn and his collaborators (2): a muscular activity will lower the cortical threshold for more of the same activity. On the neuronal level it is well known that a large depolarization (spike potential) is more easily produced in a tissue which already has a small one (local bio-electric disturbance). Prior stimulation of the muscle spindles over the "gamma system" increases their susceptibility to later stimuli (4). Some of these positively accelerated phenomena depend on positive feedback, and others do not.[5] In organismic behavior there are likewise events, such as perseveration, which suggest a positive feedback. In general, a reinforcement effect, contingent upon an action and leading to its repetition, fits quite well the model of positive feedback. Learning may thus be represented by such a model. Still more generally it is quite usual that a response is far more energetic than its stimulus. Instead of resisting a sensory disturbance, the organism amplifies it. While this is not necessarily a positive feedback, it is often hard to see that it is equilibration, either. Possibly the application of the activating and positive feedback models should be explored more than it has been.

Returning to the original question, the answer is that homeostasis would not serve as a good all-around model for behavior. Using the word in a very broad functional sense will merely enlarge the vocabulary beyond necessity. If we use it in the narrow sense of a negative feedback mechanism, we find that the model does not, and indeed cannot, fit all the facts. We can make it conform by adding a series of inventions to the facts observed, but there seems no reason for doing so. Homeostasis exists, of course, with respect to certain variables, and one should try to find out what these are. But it is to be understood as a special case of the more general conception of the response of systems to inputs. There is no compulsion to think that the organism is an elaborate machine for the purpose of getting itself back to the *status quo ante*, or, indeed, for any other purpose.

[5] A. Lundervold called my attention to the frequent occurrence of positive feedback in neural processes.

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HOW SHALL A THING BE CALLED?

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The most deliberate part of first-language teaching is the business of telling a child what each thing is called. We ordinarily speak of *the* name of a thing as if there were just one, but in fact, of course, every referent has many names. The dime in my pocket is not only a *dime*. It is also *money*, a *metal object*, a *thing*, and, moving to subordinates, it is a *1952 dime*, in fact a *particular 1952 dime* with a unique pattern of scratches, discolorations, and smooth places. When such an object is named for a very young child how is it called? It may be named *money* or *dime* but probably not *metal object*, *thing*, *1952 dime*, or *particular 1952 dime*. The dog out on the lawn is not only a *dog* but is also a *boxer*, a *quadruped*, an *animate being;* it is the *landlord's dog*, named *Prince*. How will it be identified for a child? Sometimes it will be called a *dog*, sometimes *Prince*, less often a *boxer*, and almost never a *quadruped*, or *animate being*. Listening to many adults name things for many children, I find that their choices are quite uniform and that I can anticipate them from my own inclinations. How are these choices determined and what are their consequences for the cognitive development of the child?

Adults have notions about the kind of language appropriate for use with children. Especially strong and universal is the belief that children have trouble pronouncing long names and so should always be given the shortest possible names. A word is preferable to a phrase and, among words, a monosyllable is better than a polysyllable. This predicts the preference for *dog* and *Prince* over *boxer*, *quadruped*, and *animate being*. It predicts the choice of *dime* over *metal object* and *particular 1952 dime*.

Zipf (10) has shown that the length of a word (in phonemes or syllables) is inversely related to its frequency in the printed language. Consequently the shorter names for any thing will usually also be the most frequently used names for that thing, and so it would seem that the choice of a name is usually predictable from either frequency or brevity. The monosyllables *dog* and *Prince* have much higher frequencies according to the Thorndike-Lorge list (8) than do the polysyllables *boxer*, *quadruped*, and *animate being*.

It sometimes happens, however, that the frequency-brevity principle makes the wrong prediction. The thing called a *pineapple* is also *fruit*. *Fruit* is the shorter and more frequent term, but adults will name the thing *pineapple*. Similarly they will say *apple*, *banana*, *orange*, and even *pomegranate;* all of them longer and less frequent words than the perfectly appropriate *fruit*. Brevity seems not to be the powerful determinant we had imagined. The frequency principle can survive this kind of example, but only if it is separated from counts like the Thorndike-Lorge of over-all frequency in the printed language. On the whole the word *fruit* appears more often than the word *pineapple* (and also is shorter), but we may confidently assume that, when pineapples are being named, the word *pineapple* is more frequent than the word *fruit*. This, of course, is a kind of frequency more directly relevant to our problem. Word counts of general usage are only very roughly applicable to the prediction of what will be said when

something is named. What we need is referent-name counts. We don't have them, of course, but if we had them it is easy to see that they would improve our predictions. Bananas are called *banana,* apples *apple,* and oranges *orange* more often than any of them is called *fruit.* The broad frequency-brevity principle predicts that *money* and *dime* will be preferred to *metal object, 1952 dime,* and *particular 1952 dime,* but it does not predict the neglect of the common monosyllable *thing.* For this purpose we must again appeal to imagined referent-name counts, according to which dimes would surely be called *dime* or *money* more often than *thing.*

While the conscious preference for a short name can be overcome by frequency, the preference nevertheless affects the naming act. I have heard parents designate the appropriate objects *pineapple, televsion, vinegar,* and *policeman;* all these to children who cannot reproduce polysyllabic words. Presumably they use these names because that is what the referents are usually called, but the adult's sense of the absurdity of giving such words to a child is often evident. He may smile as he says it or remark, "That's too hard for you to say, isn't it?"

Some things are named in the same way by all adults for all children. This is true of the apple and the orange. Other things have several common names, each of them used by a specifiable group of adults to specifiable children. The same dog is *dog* to most of the world and *Prince* in his own home and perhaps on his own block. The same man is a *man* to most children, *policeman* to some at some times, *Mr. Jones* to the neighborhood kids, and *papa* to his own. Referent-name counts from people in general will not predict these several usages. A still more particular name count must be imagined. The name given a thing by an adult for a child is determined by the frequency with which various names have been applied to such things in the experience of the particular adult. General referent-name counts taken from many people will predict much that the individual does, but, for a close prediction, counts specific to the individual would be needed.

The frequencies to which we are now appealing have not, of course, been recorded. We are explaining imagined preferences in names by imagined frequencies of names. It is conceivable, certainly, that some of these specific word counts might be made and a future naming performance independently predicted from a past frequency. Probably, however, such frequencies will never be known, and if we choose to explain particular naming performances by past frequencies we shall usually have to infer the frequency from the performance.

Beyond the Frequency Principle

A frequency explanation is not very satisfying even when the appeal is to known frequencies. The question will come to mind: "Why is one name more common than another?" Why is a dog called *dog* more often than *quadruped* and, by some people, called *Prince* more often than *dog?* Perhaps it just happened that way, like driving on the right side of the road in America and on the left in England. The convention is preserved but has no justification outside itself. As things have worked out, coins are usually named by species as *dime, nickel,* or *penny* while the people we know have individual names like *John, Mary,* and *Jim.* Could it just as easily be the other way around? Might we equally well give coins proper names and introduce people as types?

The referent for the word *dime* is a large class of coins. The name is equally appropriate to all members of this class.

To name a coin *dime* is to establish its equivalence, for naming purposes, with all other coins of the same denomination. This equivalence for naming purposes corresponds to a more general equivalence for all purposes of economic exchange. In the grocery one dime is as good as another but quite different from any nickel or penny. For a child the name given an object anticipates the equivalences and differences that will need to be observed in most of his dealings with such an object. To make proper denotative use of the word *dime* he must be able to distinguish members of the referent category from everything else. When he learns that, he has solved more than a language problem. He has an essential bit of equipment for doing business. The most common names for coins could not move from the species level to the level of proper names without great alteration in our nonlinguistic culture. We should all be numismatists preparing our children to recognize a particular priceless 1910 dime.

Many things are reliably given the same name by the whole community. The spoon is seldom called anything but *spoon*, although it is also a piece of *silverware*, an *artifact*, and a *particular ill-washed restaurant spoon*. The community-wide preference for the word *spoon* corresponds to the community-wide practice of treating spoons as equivalent but different from knives and forks. There are no proper names for individual spoons because their individuality seldom signifies. It is the same way with pineapples, dimes, doors, and taxicabs. The most common name for each of these categorizes them as they need to be categorized for the community's nonlinguistic purposes. The most common name is at the level of usual utility.

People and pets have individual names as well as several kinds of generic name. The individual name is routinely coined by those who are disposed to treat the referent as unique, and is available afterwards to any others who will see the uniqueness. A man at home has his own name to go with the peculiar privileges and responsibilities binding him to wife and child. But the same man who is a one-of-a-kind *papa* to his own children is simply a *man* to children at large. He is, like the other members of this large category, someone with no time to play and little tolerance for noise. In some circumstances, this same man will be given the name of his occupation. He is a *policeman* equivalent to other policemen but different from *bus drivers* and *Good Humor men*. A policeman is someone to "behave in front of" and to go to when lost. To the kids in the neighborhood the man is *Mr. Jones*, unique in his way—a crank, bad tempered, likely to shout at you if you play out in front of his house. It is the same way with dogs as with people. He may be a unique *Prince* to his owners, who feed and house him, but he is just a *dog* to the rest of the world. A homeless dog reverts to namelessness, since there is none to single him out from his species. Dimes and nickels have much the same significance for an entire society, and their usual names are fixed at this level of significance. People and pets function uniquely for some and in various generic ways for others. They have a corresponding variety of designations, but each name is at the utility level for the group that uses it. Our naming practices for coins and people correspond to our nonlinguistic practices, and it is difficult to imagine changing the one without changing the other.

The names provided by parents for children anticipate the functional structure of the child's world.[1] This is not,

[1] The equivalence of dimes and their distinctiveness as a class from nickels and pennies

of course, something parents are aware of doing. When we name a thing there does not seem to be any process of choice. Each thing has its name, just one, and that is what we give to a child. The one name is, of course, simply the usual name for us. Naming each thing in accordance with local frequencies, parents unwittingly transmit their own cognitive structures. It is a world in which *Prince* is unique among dogs and *papa* among men, *spoons* are all alike but different from *forks*. It may be a world of *bugs* (to be stepped on), of *flowers* (not to be picked), and *birds* (not to be stoned). It may be a world in which *Niggers*, like *spoons*, are all of a kind. A division of caste creates a vast categorical equivalence and a correspondingly generic name. *Mr. Jones* and *Mr. Smith* do not come out of racial anonymity until their uniqueness is appreciated.

Adults do not invariably provide a child with the name that is at the level of usual utility in the adult world. An effort is sometimes made to imagine the utilities of a child's life. Some parents will, at first, call every sort of coin *money*. This does not prepare a child to buy and sell, but then he may be too young for that. All coins are equivalent for the very young child in that they are objects not to be put into the mouth and not to be dropped down the register, and *money* anticipates that equivalence. A more differentiated terminology can wait upon the age of storegoing. Sometimes an adult is aware of a child's need for a distinction that is not coded in the English lexicon. A new chair comes into the house and is not going to be equivalent to the shabby chairs already there. A child is permitted to sit on the old chairs but will not be permitted on the new one. A distinctive name is created from the combinational resources of the language. *The new chair* or *the good chair* is not to be assimilated to *chairs* in general.

Eventually, of course, children learn many more names for each thing than the one that is most frequent and useful. Sometimes a name is supplied in order to bring forward an immediately important property of the referent. A child who starts bouncing the coffee pot needs to be told that it is *glass*. Sometimes a name is supplied to satisfy the child's curiosity as to the place of a referent in a hierarchy of categories. Chairs are *furniture* and so are tables; carrots are a *vegetable* but apples are not. Probably, however, both children and adults make some distinction among these various names. *The* name of a thing, the one that tells what it "really" is, is the name that constitutes the referent as it needs to be constituted for most purposes. The other names represent possible recategorizations useful for one or another purpose. We are even likely to feel that these recategorizations are acts of imagination, whereas the major categorization is a kind of passive recognition of the true character of the referent.

The Child's Concrete Vocabulary

It is a commonplace saying that the mind of a child is relatively "concrete"

is strongly suggested by the appearance of individual coins as well as by their names. Variations in size, weight, and hue are far greater between classes than within a class. This, of course, is because coins are manufactured in accordance with a categorical scheme which is also represented in our names for coins. It is possible, then, that a child might structure coins in the culturally approved manner if he never heard them named at all. However, we cannot be sure that an untutored child would not put all shiny new coins into one class and all the dingy specimens into another. When the referents are not manufactured articles but are such things as dogs, people, flowers, and insects, it is clear that autochthonous factors in perception do not force any single scheme of categorization. The names applied must be the child's principal clue to the locally functioning scheme.

and the mind of an adult "abstract." The words "concrete" and "abstract" are sometimes used in the sense of subordinate and superordinate. In this sense a relatively concrete mind would operate with subordinate categories and an abstract mind with superordinate categories. It is recorded in many studies of vocabulary acquisition (e.g., 2, 6) that children ordinarily use the words *milk* and *water* before the word *liquid;* the words *apple* and *orange* before *fruit; table* and *chair* before *furniture; mamma* and *daddy* before *parent* or *person;* etc. Very high-level superordinate terms like *article, action, quality,* and *relation,* though they are common in adult speech (8), are very seldom heard from preschool children (2). Presumably this kind of vocabulary comparison is one of the sources of the notion that the child's mind is more concrete than the mind of the adult.[2]

However, the vocabulary of a child is not a very direct index of his cognitive preferences. The child's vocabulary is more immediately determined by the naming practices of adults.

The occasion for a name is ordinarily some particular thing. In the naming it is categorized. The preference among possible names seems to go to the one that is most commonly applied to the referent in question. That name will ordinarily categorize the referent so as to observe the equivalences and differences that figure in its usual utilization. There are not many purposes for which all liquids are equivalent or all fruits, furniture, or parents; and so the names of these categories are less commonly used for denotation than are the names of categories subordinate to them. It is true that words like *article, action, quality* and *relation* are rather common in adult written English, but we can be sure that these frequencies in running discourse are not equaled in naming situations. Whatever the purposes for which all articles are equivalent, or all actions or qualities, they are not among the pressing needs of children.

It is not invariably true that vocabulary builds from concrete to abstract. *Fish* is likely to be learned before *perch* and *bass; house* before *bungalow* and *mansion; car* before *Chevrolet* and *Plymouth* (6). The more concrete vocabulary waits for the child to reach an age where his purposes differentiate kinds of fish and makes of cars. There is much elaborately concrete vocabulary that is not introduced until one takes courses in biology, chemistry, and botany. No one has ever proved that vocabulary builds from the concrete to the abstract

[2] From the facts of vocabulary acquisition alone it is not possible to draw safe conclusions about cognitive development. Such conclusions rely on something like the following set of assumptions. A subject, whether animal or human, is ordinarily credited with a cognitive category when he extends some distinctive response to new instances of the category and withholds it from noninstances. Words, when used to denote new referents, are such a distinctive response. If children speak words they probably can make correct denotative use of them, and so the presence of the word in a child's vocabulary may be taken as evidence that he possesses the category to which the word makes reference. The instances of the category are presumed not to be differentiated by the child unless he uses words for such differentiations. If all of these assumptions are made it would seem to follow that the direction of vocabulary growth (from subordinate to superordinate or vice versa) reveals the direction of cognitive development. When the assumptions of such an argument are explicitly stated, it is clear that they are too many and too doubtful. Obviously words may be spoken but not understood; objects may be differentiated by nonlinguistic response even though they are not differentiated linguistically. However, it is not my purpose here to quarrel with these assumptions but rather to show that, even when they are accepted, the facts of vocabulary growth do not compel the conclusion that cognitive development is from the concrete to the abstract.

more often than it builds from the abstract to the concrete. The best generalization seems to be that each thing is first given its most common name. This name seems to categorize on the level of usual utility. That level sometimes falls on the most concrete categories in a hierarchy (proper names for significant people), and vocabulary then builds toward the more abstract categories (names for ethnic groups, personality types, social classes). Utility sometimes centers on a relatively abstract level of categorization (fish) and vocabulary then builds in both directions (perch and vertebrate). Probably utility never centers on the most abstract levels (thing, substance, etc.), and so probably there is no hierarchy within which vocabulary builds in an exclusively concrete direction.

In the literature describing first-language acquisition (**5**) there is much to indicate that children easily form large abstract categories. There are, to begin with, the numerous cases in which the child overgeneralizes the use of a conventional word. The word *dog* may, at first, be applied to every kind of four-legged animal. It sometimes happens that every man who comes into the house is called *daddy*. When children invent their own words, these often have an enormous semantic range. Wilhelm Stern's (**7**) son Günther used *psee* for leaves, trees, and flowers. He used *bebau* for all animals. Lombroso (**9**) tells of a child who used *qua qua* for both duck and water and *afta* for drinking glass, the contents of a glass, and a pane of glass. Reports of this kind do not suggest that children are deficient in abstracting ability. It even looks as if they may favor large categories.

There are two extreme opinions about the direction of cognitive development. There are those who suppose that we begin by discriminating to the limits of our sensory acuity, seizing each thing in its uniqueness, noting every hair and flea of the particular dog. Cognitive development involves neglect of detail, abstracting from particulars so as to group similars into categories. By this view abstraction is a mature rather than a primitive process. The contrary opinion is that the primitive stage in cognition is one of a comparative lack of differentiation. Probably certain distinctions are inescapable; the difference between a loud noise and near silence, between a bright contour and a dark ground, etc. These inevitable discriminations divide the perceived world into a small number of very large (abstract) categories. Cognitive development is increasing differentiation. The more distinctions we make, the more categories we have and the smaller (more concrete) these are. I think the latter view is favored in psychology today. While there is good empirical and theoretical support (**1**, **3**, **4**) for the view that development is differentiation, there is embarrassment for it in the fact that much vocabulary growth is from the concrete to the abstract. This embarrassment can be eliminated.

Suppose a very young child applies the word *dog* to every four-legged creature he sees. He may have abstracted a limited set of attributes and created a large category, but his abstraction will not show up in his vocabulary. Parents will not provide him with a conventional name for his category, e.g., *quadruped*, but instead will require him to narrow his use of *dog* to its proper range. Suppose a child calls all elderly ladies *aunt*. He will not be told that the usual name for his category is *elderly ladies* but, instead, will be taught to cut back *aunt* to accord with standard usage. In short, the sequence in which words are acquired is set by adults rather than

children, and may ultimately be determined by the utility of the various categorizations. This will sometimes result in a movement of vocabulary toward higher abstraction and sometimes a movement toward greater concreteness. The cognitive development of the child may nevertheless always take the direction of increasing differentiation or concreteness.

The child who spontaneously hits on the category four-legged animals will be required to give it up in favor of dogs, cats, horses, cows, and the like. When the names of numerous subordinates have been mastered, he may be given the name *quadruped* for the superordinate. This abstraction is not the same as its primitive forerunner. The schoolboy who learns the word *quadruped* has abstracted from differentiated and named subordinates. The child he was abstracted through a failure to differentiate. Abstraction after differentiation may be the mature process, and abstraction from a failure to differentiate the primitive. Needless to say, the abstractions occurring on the two levels need not be coincident, as they are in our quadruped example.

SUMMARY

Though we often think of each thing as having a name—a single name—in fact, each thing has many equally correct names. When some thing is named for a child, adults show considerable regularity in their preference for one of the many possible names. This paper is addressed to the question: "What determines the name given to a child for a thing?" The first answer is that adults prefer the shorter to the longer expression. This gives way to the frequency principle. Adults give a thing the name it is most commonly given. We have now come full circle and are left with the question, "Why is one name for a thing more common than another?"

It seems likely that things are first named so as to categorize them in a maximally useful way. For most purposes Referent A is a spoon rather than a piece of silverware, and Referent B a dime rather than a metal object. The same referent may have its most useful categorization on one level (*Prince*) for one group (the family) and on another level (*dog*) for another group (strangers). The categorization that is most useful for very young children (*money*) may change as they grow older (*dime* and *nickel*).

With some hierarchies of vocabulary the more concrete terms are learned before the abstract; probably the most abstract terms are never learned first, but it often happens that a hierarchy develops in both directions from a middle level of abstraction. Psychologists who believe that mental development is from the abstract to the concrete, from a lack of differentiation to increased differentiation, have been embarrassed by the fact that vocabulary often builds in the opposite direction. This fact need not trouble them, since the sequence in which words are acquired is not determined by the cognitive preferences of children so much as by the naming practices of adults.

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"WHAT IS LEARNED?"—AN EMPIRICAL ENIGMA[1]

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Although cognitive and stimulus-response interpretations of learning have traditionally been regarded as antipodes in the theoretical disputes of behavioral psychology, it has become increasingly apparent that there are few, if any, predictive *behavioral* differences between these positions. Superficially, one might expect gross disagreement between an S-R theory, which holds to a direct attachment of responses to stimuli, and a cognitive or "expectancy" theory which maintains that conditioned stimuli elicit sensory processes, or surrogates thereof, previously associated with other stimuli. Yet when one reflects that the concept of "mediation response" (27) or "anticipatory goal response" (15) allows a stimulus to evoke a CR whose proprioceptive feedback provides a mechanism by which a stimulus may have conditioned sensory consequences, it is no longer surprising that no one has yet succeeded in cooking up a "critical" experiment to settle the controversy.

It is entirely too rash, however, to conclude with Kendler (20) that speculations as to "what is learned" are empirically meaningless. To do so is to overlook the crucial point that the known —or supposed—facts of behavior have driven S-R theorists from their older models *without* mediation responses to a more complex theoretical analysis which is, within rather wide limits, *formally* identical with expectancy theory. There *are* important predictive differences between the old, unmediated, S-R theory on the one hand, and mediated S-R or expectancy theory on the other. Hence, if by "What is learned?" we are concerned not with the connotative imagery associated with theoretical terms but with the formal properties of learning theories, it would appear that this question does have empirical significance. The fact that behavior theories of widely divergent conceptual origins appear rapidly to be converging to a common formal structure strongly implies the existence of certain *empirical* principles of behavior which necessitate this particular structure. And if such is indeed the case, we are confronted with a problem: Exactly what *are* the empirical principles which give rise to mediational theories of learning?

The question just posed is not as simple as it might appear. Every graduate student of behavioristics is familiar with, e.g., the "latent learning" experiments which were supposedly critical for expectancy theories, and whose explanations in S-R terms draw so heavily on mediation responses. But an experiment is not an *empirical principle;* by the latter, we understand a lawful relation among observation variables which, in conjunction with other empirical principles, determines the outcome of an experiment. To understand the empirical bases for mediation hypotheses, it is insufficient to know what experimental outcomes favor a mediation hypothesis. We must also know the laws which these results exemplify.

Let me elaborate on this point, for it is an important one. There appear to

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be two main approaches through which one can seek understanding of natural phenomena. One—the theoretic—is to hypothesize about the underlying unobserved entities or processes and their relations which are casually responsible for the gross observed effects. The other—the empiristic—is to seek discovery of a set of dependable empirical covariations, in terms of which more complex phenomena may then be analyzed. There is no need here to weigh the relative merits of these approaches.[2] What is important to note is that there is no reason to doubt that any behavioral phenomenon can be analyzed as an instance of one or more purely empirical principles, even though these, in turn, may call for explanation in theoretical terms. The importance of such "brute facts" of behavior for empiricist and theorist alike is appreciated readily enough by reflection on the position in behavioral psychology of the empirical laws of conditioning, both classical and instrumental, the facts about deprivation and activity level, etc. Such principles may be regarded as what we *know* about behavior, in contrast to what we *surmise* (though, of course, in the last analysis this is less an absolute distinction than a matter of degree), and it is this hard core of *facts* —by which I mean not merely data, which are gathered readily enough, but the much less obvious empirical covariations which these exemplify—upon which the *science* of behavioristics is erected.

The question which I have raised is whether the problem, "What is learned?", is not reflected on the empirical level as a question about the laws of behavior. I have already argued that the structural convergence of alternate learning theories does, in fact, suggest this to be so. The question still remains, however, as to whether the particular relations responsible for mediation hypotheses are merely special instances of laws already known, *or whether, perhaps, a previously unrecognized basic principle of behavior is involved.* I shall attempt to demonstrate that the latter is, indeed, the case —or more accurately, that the empirical basis for mediation hypotheses is a fundamental behavioral principle *if,* in fact, it exists. For, as will be shown, the problem at hand carries us into a new, essentially unexplored, dimension of research.

I shall not attempt to display the empirical problem of "What is learned?" through analysis of the traditional latent learning and reasoning experiments. To do so would be to become lost in a mass of obscuring details, and to suggest that the principle for which we are seeking manifests itself only in special circumstances. Rather, I shall demonstrate that *the empirical possibility which gives rise to mediation theories of learning is an intrinsic aspect of each and every instance of conditioning phenomena.* The known facts of conditioning are traditionally phrased in a way that conceals a highly critical ambiguity concerning what *are* the facts of conditioning. In the next section, I shall attempt to formulate the conditioning paradigm in complete generality, and to exhibit the correlated empirical mediation problem, without whose answer a resume of the facts of conditioning is as incomplete as a specification of geographical location which gives latitude but not longitude. But to illustrate my point and to illuminate the general analysis, I will first cite a specific example.

Perhaps the oldest empirical principle of behavioristics is that of classical conditioning. This may be expressed:

(I) *If an organism repeatedly encounters, under suitable circumstances, a stimulus,* S_c, *immediately preceding a*

[2] If space permitted, I would argue that theoretical concepts are—within sharply circumscribable limits—not merely justified, but are more or less *compelled* by certain kinds of empirical data.

second stimulus, S_u, *which evokes response* R, *then* S_c *will come to evoke* R.

With some qualifications (chiefly in regard to the similarity of CR and UR), (I) may stand as a close paraphrase of conventional formulations of classical conditioning, and *seems* to assert no more than is justified by generalization from known data. But compare it with the following formulation:

(II) *If an organism repeatedly encounters, under suitable circumstances, a stimulus,* S_c, *immediately preceding a second stimulus,* S_u, *then* S_c *will come to evoke the same response(s) as elicited by* S_u.

Both (I) and (II) are equally supported by the facts of conditioning, so far as these are known; *yet* (I) *implies that classical conditioning establishes a bond between* S_c *and* R *which is independent of the subsequent behavioral effects of* S_u, *whereas* (II) *implies that any modifications in the response-evocation properties of* S_u *will be passed along to* S_c—*i.e., that classical conditioning establishes a functional equivalence between conditioned and unconditioned stimulus.* Since the behavioral effects of the US have almost invariably been left unaltered in conditioning experiments, there is absolutely no justification for assuming (I) to be any more representative of the empirical facts than (II). Yet it is traditionally assumed by empiricists and S-R theorists that the conditioned reflex is a "direct" or "mechanical" attachment of response to stimulus—i.e., that (I) is the case. The truth is that we *simply don't know* what the relationship is between the responses to the CS and US subsequent to the conditioning operation, despite the fact that the value of this information, both for theoretical and applied purposes, can scarcely be overestimated.

The moral that I want to draw is that our knowledge of the empirical facts of classical conditioning is incomplete—not in the relatively trivial sense that we are not yet acquainted with all the parameters of its occurrence, but in the extremely basic sense that we have only partial knowledge of its empirical *consequences.* Moreover, unless we explicitly stipulate that the behavioral effects of the US remain constant, it appears extremely difficult to express what we do know about conditioning without inadvertently prejudging the results of experiments yet to be conducted. (Such prejudgments undoubtedly account for our traditional assumption that a conditioned reflex, once established, is independent of the subsequent effects of the US.) But the relation between the response-evocation properties of the CS and those of the US constitutes an empirical principle yet to be determined, and it is the likelihood that a significant relationship does in fact exist, along with others of its kind, that has encouraged the development of mediational theories of learning.

THE GENERALIZED CONDITIONING PARADIGM

The preceding example demonstrates how, for classical conditioning, the problem, "What is learned?", is not primarily a theoretical issue at all, but a straightforward query about the brute facts of behavior. As we shall now see, a similar problem exists for every form of conditioning. To show this in complete generality, it is first expedient to assimilate the various instances of conditioning—conditioned reflex, instrumental conditioning, conditioned reinforcement, etc.—under a single rubric.

Most, if not all, of those instances of learning to which the term "conditioning" has been applied, as well as foreseeable extensions of this term, may be subsumed under the formula of a stimulus, S_c, acquiring certain behavioral effects (for a given organism) through participation of S_c in certain contin-

gency relations with other environmental events. By "behavioral effect" I mean any change, transient or enduring, observed or inferred, in the behavioral attributes of an organism which results from reception of the stimulus by that organism. The simplest and best known behavioral effect of a stimulus is, of course, response evocation, but more complex consequences of stimuli for behavior are also known. One such effect is (instrumental) "reinforcement," which confers upon a stimulus the ability to produce instrumental conditioning. Still another is drive arousal or reduction, which differs from simple response evocation in that the former is characterized by a change in activity level which may be expressed in a variety of behaviors. There is evidence that these effects may be acquired and lost in manners vary similar to the acquisition and loss of response evocation.

Since environmental events are presented to the organism in terms of stimuli,[3] we may formulate the conditioning schema as follows: *A phenomenon is known as "conditioning" when it may be described as the acquisition by a stimulus,* S_c, *of a behavioral effect,* E_c, *through participation of* S_c *in a contingency relation,* $T(S_c, S_u)$, *with an "unconditioned" stimulus,* S_u, *whose behavioral effects are* E_u. (Although the origins of the term "unconditioned stimulus" lie in the innate autonomic reflexes of Pavlovian conditioning, it is terminologically convenient to regard the stimulus through which conditioning is accomplished as an unconditioned stimulus, even though it may have received its own behavioral effects through a previous conditioning procedure.[4]) Let "E is a behavioral effect of stimulus S" be abbreviated "$S \rightarrow E$." Then if $S_u \rightarrow E_u$ and $T(S_c, S_u)$ together causally imply $S_c \rightarrow E_c$, $T(S_c, S_u)$ is known as a "conditioning operation," while if $S_c \rightarrow E_c$ is due to a conditioning operation, S_c and E_c are a "conditioned stimulus" and a "conditioned effect," respectively. The role of E_u in the conditioning procedure seems to be that of determining the nature of the conditioned effect, E_c, since the conditioning powers of a given S_u are altered as its E_u is altered.

It must clearly be appreciated that the generalized conditioning paradigm, so formulated, in no way asserts a *law;* it merely describes those generic empirical features which are exemplified by conditioning phenomena. So far, nothing has been said about the kinds of behavioral effects involved, nor have we specified any details of the conditioning operation, $T(S_c, S_u)$. There will be as many species of conditioning as exist behavioral effects and contingency relations with the appropriate consequences. Neither, as yet, has the relation between the unconditioned effect, E_u, and the conditioned effect, E_c, been specified; we shall turn to a detailed analysis of this following a brief summary of the two major known types of conditioning operations.

*Classical, Pavlovian, or type-*S *conditioning.* In this instance, the occurrence of the unconditioned stimulus, S_u, is made contingent only upon the occurrence of the conditioned stimulus, S_c. So far as is known, classical conditioning can be accomplished whether the behavioral effect of S_u is innate or itself conditioned, though for technical reasons, higher order classical conditioning is difficult to demonstrate empirically

[3] Since *behavioral* manipulations of the organism consist of *environmental* modifications, the independent variables of behavioristics, including drive operations, can always be formulated in terms of stimulus situations, although certain methodological complexities arise in the case of the deprivations.

[4] It would be preferable to call S_u the "conditioning" stimulus, but this makes for excessive confusion with "conditioned" stimulus.

(35, p. 244 f.), and is, perhaps, but weakly established (28). With the behavioral effects of S_u held constant, the conditioned effect, E_c, appears to be similar to the unconditioned effect, E_u, when E_u is response evocation, positive or negative reinforcement, or drive evocation (e.g., 7, 9, 13, 19, 24, 35). Available evidence (e.g., 6, 22) tends to weigh against the conditioned acquisition of drive reduction.

*Instrumental, or type-*R *conditioning.* When the occurrence or cessation of a stimulus, S_u, is made contingent upon emission of a response, R, in the presence of a stimulus, S_c—i.e., the occurrence or cessation of S_u is dependent on the joint occurrence of S_c and R—the subsequent likelihood of R as a response in the presence of S_c may be altered, depending upon the reinforcement value of S_u (e.g., 13, 19, 35). When the onset of S_u contingent upon R in the presence of S_c results in the increment of R as a response tendency to S_c, then S_u is known as a "positive" reinforcer. When it is the termination of S_u that strengthens $S_c \rightarrow R$, then S_u is termed a "negative" reinforcer. The onset of a negative reinforcer contingent upon R in the presence of S_c is known to decrease the strength of $S_c \rightarrow R$, perhaps through instrumental strengthening of competing responses (e.g., 11). During instrumental conditioning, the reinforcement value and probably other behavioral effects (other than drive reduction) of the reinforcing stimulus also apparently tend to become conditioned to S_c, undoubtedly through a type-S process. Thus R is only a portion of the total effects, E_c, acquired by S_c during instrumental conditioning. (Note, incidentally, that classical conditioning may be conceived as a special, or degenerate, case of instrumental conditioning in which the instrumental response, R, is the null-response—i.e., S_u is contingent upon S_c plus no particular response.) There is still much that is unknown about the parameters of instrumental conditioning, particularly in regard to the participation of drive.

THE PROBLEM OF CONDITIONED GENERALIZATION

In predicting the behavioral effects of a conditioned stimulus, it is insufficient to know merely the stimulus contingencies in which S_c has participated; we also need to know the behavioral effects of S_u. That is, E_c is a function not merely of $T(S_c, S_u)$, but also of E_u. But what do we know of this function empirically?

The extent of our knowledge here has already been summarized (perhaps overtersely) above. We know that *so long as the behavioral effects of* S_u *remain constant,* roughly $E_c = E_u$ for classical conditioning,[5] and roughly $E_c = R + E_u$ for instrumental conditioning, where R is the instrumental response (or, in certain instances, its inhibition). But this leaves unspecified *which* behavioral effects of S_u determine E_c, for, in principle, the effects of S_u can vary with time. At a given moment, are the conditioned effects of S_c determined by the effects of S_u *at that same moment,* or by the effects of S_u *at the time of conditioning?* (There are, of course, still other possibilities.) Either alternative has as

[5] Cognitive theorists have shown an inordinate concern over the apparent fact that the classically conditioned response tends to differ somewhat from the unconditioned response. There is no more a priori reason why E_c should be identical with E_u than some other fixed function of E_u at the time of conditioning—neither is any less "mechanical" than the other. The difference between E_c and E_u lends no more special credence to expectancy theories than does the even greater lack of similarity between E_c and E_u during instrumental conditioning. What *is* important for a mediation theory is whether the behavioral effect of S_c, *whatever* this may be when E_u is held constant, shows any tendency to covary with E_u after conditioning.

much a priori likelihood as the other, and only data from experiments in which the effects of S_u are altered subsequent to conditioning will permit decision betwen them.

The analysis so far has been conducted in a quasi-formalistic manner in an effort to exhibit the problem of the functional dependence of conditioned effect upon unconditioned effect as, logically, an intrinsic aspect of conditioning phenomena. Let me now put it straightforwardly. *When a stimulus,* S_c, *is conditioned to a behavioral effect,* E_c, *through contingencies of* S_c *with an unconditioned stimulus,* S_u, *whose behavioral effect is* E_u, *what is the functional dependence, if any, of* E_c *upon* E_u *after conditioning?* For greater clarity, we may formulate the problem as an experimental paradigm.

Phase 1. Condition a behavioral effect, E_c, to a neutral stimulus, S_c, by making stimulus S_u, with behavioral effect E_u, contingent upon S_c alone, or upon a response, R, in the presence of S_c.

Phase 2. Extinguish E_u as an effect of S_u and condition to S_u a new effect, E'_u. Extinction of E_u is not essential, since the objective here is only to modify the effects of S_u. However, extinction of E_u, when this is possible, tends to maximize the difference in the effects of S_u before and after modification, and hence to maximize evaluation of the results in Phase 3.

Phase 3. Test S_c for its influences on behavior.

There are three major alternative results which might be anticipated as the outcome in Phase 3. These are not logically exhaustive, but would seem to cover the intuitive expectations of the behaviorist.

A. Stimulus S_c may continue to have E_c as its behavioral effect, and have gained no new effect. This indicates that under the parameters of the experiment, the behavioral effects of a CS are independent of the subsequent effects of the US. Such a result is commensurate with a nonmediational theory of learning.

B. S_c may have lost E_c as an effect, but instead, have acquired effect E'_c, where E'_c is that behavioral effect which S_c would have acquired during the initial conditioning had E'_u, rather than E_u, been the effect of S_u at that time. This would imply that, under the parameters of the experiment, the behavioral effects of a conditioned stimulus continue to be determined by those of the stimulus through which conditioning was achieved, so that; empirically speaking, the conditioning operation has established a stimulus-stimulus relationship in that the organism's responses to S_c take account, so to speak, of the current behavioral significance of S_u. Theoretically, this would imply that the relation between a CS and its effect is mediated by a process whose behavioral properties are under control of the US.

C. S_c may retain effect E_c, perhaps more or less attenuated, and also acquire some degree of E'_c, thus implying that the effects of S_c are only partly dependent upon those of S_u under these parameters. The correlated theoretical interpretation is that the behavioral effects of the CS are multidetermined, some involving a mediation process controlled by the US, and others being independent of it.

For convenient reference, it is desirable to find a distinctive title for the experimental paradigm just described. Since Outcomes B and C display a dependence of the behavioral effects of one stimulus upon those of another as the result of a conditioning procedure, the name "conditioned effect dependency" provides a literal description but is unpleasantly cumbersome. However, when the effects E_c and E'_c of the conditioned stimulus are similar, respectively, to

those, E_u and E'_u, of the unconditioned stimulus, the much simpler term, "conditioned generalization," is also appropriate. For the sake of descriptive expediency, therefore, let me stretch a point and refer to the generic design as the *conditioned generalization* paradigm, even though the effects of the CS and US need not literally be the same.

There are several important observations to be made here. First, let me emphasize once again that although the conditioned generalization paradigm is distinctive as an experimental procedure, the empirical question which it is designed to answer is *not* a problem separate from or secondary to the facts of conditioning. Quite the contrary, it is a question *about* the facts of conditioning. As was illustrated earlier for the conditioned reflex, the empirical laws of conditioning are traditionally formulated in a way that negatively prejudges the existence of conditioned generalization, with the result that any experiment which appears to exemplify this effect is regarded as a special and complex instance to be explained away, if possible, in terms of more "fundamental" empirical principles. But from the available data, there is no more reason to think that the primary empirical consequence of the conditioning operation is establishment of a rigid $S_c \rightarrow E_c$ connection than it is the formation of a generalization from S_u to S_c. We simply do not yet know what the facts are.

Secondly, it is important to realize that the question of conditioned generalization is not a unitary problem to be answered by a single experiment, but a correlate to *every instance* of conditioning. There is no particular reason to think that the degree of conditioned generalization must be the same for every type of conditioning. In fact, judging from what data there are, it seems probable that the degree of conditioned generalization is not constant even for a given type of conditioning, but is highly dependent upon the particular parameters of the learning situation.

The next point is an extension of the first two. Since every type of conditioning is a conditioned generalization phenomenon (if only in a trivial sense that E_c is a constant function of E_u), the problem of conditioned generalization is quite literally a new *dimension* in the empirical study of learning, rather than merely an unexplored sector of an old framework. As such, the empirical principles which will emerge from the experimental study of this problem are bound to have powerful repercussions for our understanding of behavior. Not merely are the empirical facts of readily foreseeable applied value; data concerning the dependence of conditioned generalization upon parameters yet to be discovered will also provide a new set of observation variables intimately correlated with intra-organismic processes for which at present we have only tenuous theoretical speculations. It is perhaps not unfair to claim that conditioned generalization is one of the most important problems now confronting behavioristics.

Finally, it should be apparent that it is the facts of conditioned generalization, whatever they may be, which solve the old enigma, "What is learned?" [6] Far from being a theoretical blind alley, this is a straightforward question about the empirical consequences of the conditioning operation.

[6] Some writers (e.g., 21) have interpreted the question, "What is learned?", as concerning whether the conditioned response is a motor discharge or an achievement. However, this would seem to be more a generic problem in regard to the nature of the response than about learning as such. Since, as MacCorquodale and Meehl (21, p. 219) point out, S-R theorists are no more committed to, and in the main have no more subscribed to, a motor discharge conceptualization of the response than have cognitivists; the *dispute* over what is *learned* must lie in something other than this.

THE EVIDENTIAL BASIS FOR CONDITIONED GENERALIZATION

I have contended that it is belief in a principle of conditioned generalization that has motivated development of mediation devices such as expectancies and fractional anticipatory goal responses. Since rigorous demonstration of the dependency of theoretical mediation elements upon this kind of empirical covariation calls for a technical and fairly extensive logical analysis, I shall instead substantiate this claim by showing that a large proportion of those experiments traditionally cited as critical for mediation theories are instances of the conditioned generalization paradigm. These may be grouped into three major categories: (*a*) "secondary generalization," (*b*) "preconditioning," and (*c*) a more loosely organized set of experiments which may be called "secondary instrumental conditioning."

Secondary generalization. In 1933, Shipley (32) reported that if, for human subjects, an eyeblink is conditioned to a flash of light with a tap on the cheek as the US, and a finger flexion is subsequently conditioned to the tap (shock as the US), the light flash will be found to evoke finger flexion. On the basis of this single experiment, Hull (16, 17) proclaimed the principle of "secondary generalization," which hypothesizes that if two stimuli, S_1 and S_2, evoke the same response, R, then another response, R', conditioned to one of the stimuli, say S_1, will also be evoked by the other, S_2. This formulation differs somewhat from that of conditioned generalization in that, for secondary generalization, S_1 and S_2 have not necessarily had to participate in a common contingency relation. However, the case of secondary generalization, wherein the stimulus S_1, to which R' is subsequently conditioned, has been used previously as the US for conditioning R to S_2, as is true in the Shipley experiment,[7] is also, obviously, an instance of the conditioned generalization paradigm.

Shipley's experiment and results have been successfully repeated by Lumsdaine (13, p. 230), and a similar design by Graham (12), with dogs, also showed positive results. Thus the evidence is rather convincing that at least *some* degree of conditioned generalization is sometimes established during classical conditioning, although Razran (28, p. 328) cites certain Russian studies that cast doubt on its ubiquity. These experiments do not permit evaluation of the strength of the effect, however; in particular, they do not permit decision between alternatives B and C of the conditioned generalization paradigm, nor whether conditioned generalization is the rule, rather than the exception, for classical conditioning. It should be added that the Shipley-Lumsdaine-Graham experiments appear to be the only available evidence directly relevant to the secondary generalization hypothesis, despite the important role this supposedly empirical principle has played in mediated S-R theory.[8] The notion of sec-

[7] Hull (16; 17, p. 192 f.) discusses Shipley's 1933 data as though Shipley had not conditioned finger flexion to tap on cheek, and as though transfer of finger flexion to light flash were due to shock evoking both eyeblink and finger flexion. Such an interpretation would be highly favorable to the existence of secondary generalization and the response theory of mediation, but it is definitely *not* in accord with the facts of the experiment.

[8] A later (1935) experiment by Shipley (33), in which S_1 and S_2 were separately conditioned to R, appears to support the existence of secondary generalization as distinct from conditioned generalization. However, close analysis of the control data in this and the 1933 experiment reveals the 1935 results to be ambiguous. The preconditioning experiment by Wickens and Briggs (38), mentioned later, might be interpreted as an instance of secondary generalization, but only if we are willing to regard human behavior in which a stimulus elicits a response as the result of verbal in-

ondary generalization has been occasionally invoked in other experimental contexts, such as generalization gradients among symbolically related stimuli, but more as an explanatory device than an object of experimental demonstration. Thus there are no grounds for believing in a principle of secondary generalization as distinct from conditioned generalization.

Preconditioning. The preconditioning paradigm begins with repeated pairings of two supposedly neutral stimuli, S_1 and S_2, and subsequently conditions a response, R, to one of them, say S_1. Preconditioning is shown if S_2 now also tends to evoke R. The preconditioning design is a special case of the conditioned generalization paradigm in that the behavioral effect of S_1 during the pairings of S_1 and S_2 is the null-effect, or, more likely, an unidentified effect. This phenomenon was demonstrated fairly conclusively by Brogden in 1939 (2). Since then, a number of preconditioning experiments have appeared in the literature (1, 3, 4, 8, 18, 34, 38), all but one (3) reporting positive findings. But, the claims of Bitterman (1) notwithstanding, any attempt to evaluate the magnitude of the effect strongly suggests that the phenomenon is a minor one, at least judging from the published studies. Thus the empirical importance of preconditioning is still problematic.

Secondary instrumental conditioning. In this category, we may include those experiments which, following establishment of an instrumental response, modify the behavioral effects of the reinforcing stimulus. More specifically, the problem may be phrased: If an instrumental response is conditioned to a stimulus, S_c, through reinforcement by a stimulus, S_u, and the reinforcement value of S_u is now altered, will the response to S_c remain unaffected, or will it have adapted in a manner commensurate with the new behavioral significance of S_u? When S_u is positively reinforcing during conditioning, and is subsequently extinguished of positive value or aversively reconditioned, this design may also be called "secondary extinction" (29), "latent extinction" (31), or "nonresponse-extinction" (10). A typical secondary extinction experiment conditions, say, a position habit in a T maze, one goal box of which contains food or water. The animal is then placed for a while directly in the positive goal box, which is now empty of the primary reinforcer, and where the animal may also be shocked. The experimental question is whether the position habit will appear weakened on the next trial. The habit may be analyzed as a response chain held together by secondary reinforcement deriving from the sequence of discriminative cues. Reconditioning directly in the goal box tends to extinguish or aversively recondition the terminal reinforcers of the sequence, and the next maze trial reveals the immediate effect on earlier links of the response chain.

An experiment of this sort was first proposed by Tolman in 1933 (37), who obtained negative results. But shortly thereafter, Miller (23) reported that shocking rats placed directly in a distinctive goal box inhibited a running response which had previously terminated in that goal box. Secondary extinction research subsequently languished until the appearance of a number of recent studies (5, 10, 26, 29, 30, 31), most of which, unfortunately, have technical flaws (cf. 26, 29) which vitiate their interpretive significance. However, the evidence seems fairly definite that, under at least some parameters of conditioning, a lever-pressing habit in rats is quite independent of the subsequent

structions as an instance of "response evocation" in the same sense that we apply this phrase to infra-human behavior.

value of the reinforcers through which the habit was established (29).

An important variety of traditional latent learning research (*type* 5 [21, p. 211]) may also be reduced to the secondary-instrumental-conditioning rubric. In this instance, the organism is given experience with a maze, with eliminable reinforcers either absent, or equally distributed to all end boxes. The animal is now introduced directly into one end box and given a previously unencountered reinforcement, either positive or negative. The problem is whether or not the animal's maze behavior will have adapted to the changed conditions on the next trial. This differs from more obvious cases of secondary instrumental conditioning only in that the role in establishment of initial maze habits played by the stimuli whose secondary reinforcement values are subsequently modified is not so clear. When no known reinforcers are obviously influential in the initial maze behavior, this design may be described as "instrumental preconditioning," since it involves a stimulus, S_u (say, an end box), as contingent on a response, R, in the presence of another stimulus, S_c (the antecedent portion of the maze), but where S_u has no readily identifiable reinforcement value prior to the reconditioning operation. A number of experiments of this sort have been reported (see 21, p. 211, for summary; also, 14, 25, 36), with outcomes about equally divided between positive and negative. Thus again, the evidence appears to favor the existence of conditioned generalization under some circumstances, but more explicit evaluation of the empirical significance of these experiments is precluded by the difficulties in giving an unambiguous account of the initial maze learning in terms of conditioning principles.

In summary of the available evidence concerning conditioned generalization, it would thus appear that while the quantity of relevant data is very meager, they distinctly imply that parameters of conditioning exist under which at least *some* degree of correlation between the behavioral effects of CS and US is established. Unfortunately, the existent experiments happen, for the most part, to be of special designs which either employ a minimal or unknown unconditioned effect, E_u, or do not compare the relative strengths of E_c and E'_c subsequent to reconditioning of S_u. It is only when E_u and E'_u are strong and well defined, and the alternatives E_c and E'_c are distinct and quantifiable, that we can decide whether the *primary* result of a conditioning operation is establishment of a full-fledged generalization from US to CS, or whether the prevalent notion that conditioning attaches a specific effect to the CS is basically correct, and apparent cases of conditioned generalization are secondary phenomena to be explained as special instances of more fundamental empirical principles. It is earnestly to be hoped that the present analysis, by stripping from the empirical question of "What is learned?" its obscuring theoretical superstructure, and developing a general experimental paradigm which isolates the essential features of the problem, will stimulate research intended not so much to test a theoretical prediction as simply to learn the facts.

SUMMARY

I have contended that, far from being a blind alley, the question "What is learned?" is an extremely important empirical problem about the consequences of conditioning, and one which has been concealed by improper formulations of the known facts. We have traditionally assumed that a conditioning operation attaches a fixed behavioral effect, E_c, to the conditioned stimulus, S_c, whereas in fact, the conclusion that S_c will give

rise to E_c subsequent to conditioning *is empirically justified only so long as the behavioral effects of the unconditioned stimulus,* S_u, *remain constant.* The question then arises as to the extent to which, subsequent to the conditioning operation, the effects of S_c are a function of those of S_u. I have elected to identify this as the problem of "conditioned generalization," since, in one of its forms, it asks whether or not a conditioning operation establishes a generalization from US to CS.

Since the empirical consequences of the conditioning operation constitute a problem extending over the entire range of conditioning phenomena, and one which has remained unexplored in the overwhelming majority of conditioning experiments, conditioned generalization defines a new dimension of behavioral research. What few data are available suggest that conditioned generalization does exist as a phenomenon, but are totally inadequate as a basis for judging whether it is a basic behavioral principle or merely a secondary effect. Because of the practical value of this information, and its crucial significance for the development of behavior theory, it is to be hoped that the conditioned generalization problem will soon be subjected to a concerted experimental attack.

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PERCEPTION: EVOLUTION OF A CONCEPT[1]

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This paper is essentially an examination of theories of perception—especially in their relation to general behavior theory. If it at times appears to be little more than verbal masonry, this is because defining such a relationship is, in many respects, at present a semantic problem resolvable in terms of personal preference.

Reference to general behavior theory typically implies formulations like Hull's behavior principles applied to maze learning, the learning of nonsense syllables, brightness and form discrimination, social learning, emotional disturbance, and certain perceptual phenomena, or the Gestalt principles of perceptual organization applied as an analogue to explain problem solving, and even the organization of overt responses. These contrast sharply with theories like Granit's dominator-modulator theory, and the Wever-Bray volley-principle which were devised to account for sense-organ function.

The relation of perceptual to general behavior theory. The nexus between the two classes of theory under consideration is typically delineated on a background of metaphysical bias, though this is often confused by semantic issues. Some theorists have contended, as did the early behaviorists, that behavior is objective (publicly observable) and physical (involves muscles and glands), while perception is subjective (accessible only to the pereceiver) and mental (involves conscious experience); and, since scientific theories require objective data, perceptual theories *ipso facto* are not scientific. Thus there is no inter-theory relationship to describe. This position is predicated on the assumed synonymity of "objective" and "publicly observable." An alternative position is to include as objective inferred events that stand in a logically consistent relationship with the publicly observable. This means that the objectivity of a concept is determined by the efficiency with which it yields *reliable* replications of observations (9). Perception thus qualifies as an aspect of behavior, though always an inferred aspect. Indeed, if one extends this line of reasoning, one must conclude that no instance of behavior is directly observed. Rather, it is always inferred by an intelligent observer. It is meaningful (i.e., evident as behavior) to the extent that the data of observation are similar in some fashion to data which the observer has previously used in constructing his conception of the world (1). It is, of course, not incompatible with this view to argue that gross bodily movements, since they do not require transformation by the subject into verbal form before becoming available data, are more objective than perceptual data.

Having committed oneself to this latter view—as I have—one is still faced with the necessity of defining the relationship between perception and other aspects of behavior. The early psychophysicists viewed perception as a simple mediating process—a constant in the transformation of information received

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by the organism from a form that exists external to the organism to a form that is utilizable in overt response. Thus considered, there is no problem of perception beyond that of translating into psychological terms—or, as some prefer, into physiological terms—the characteristics of the external world. Once the constants of transformation have been determined (cps expressed as mels, db as sones, millilamberts as brils, grams as vegs, texture gradient as slant, etc.), the perceptionist's task is complete. Indeed, since perception does not dynamically affect other aspects of behavior, one may ignore perception and correlate stimulus input with some aspect of overt response.

Recent observers insist, however, that perception is a function not only of present stimulus input and receptor function, but also of motivation (which has stimulus properties) and past stimulation—what Helson (34) calls *residual stimuli*. Thus the perceptual mechanism is a complex interactive one, and the identification and definition of the functions that influence it become indispensable to an understanding and prediction of both perception and overt response.

That understanding perception is important to an understanding of overt behavior is an insight shared by modern phenomenological and behavior theorists. Approximately 25% of the articles in the PSYCHOLOGICAL REVIEW over the past five years have been on perception. Floyd Allport (4), known for years as an uncompromising behaviorist of the unsentimental type, has written in the preface of his volume reviewing perceptual theories: "In them, if they are taken all together, lie the essentials of our present knowledge of one of the most important activities of living beings." In similar vein are Woodworth's assertion that "the conditioning experiment is really concerned with the establishment of a new perception" (73, p. 124), and Hilgard's statement that "the learning theorist must be aware of the learning within perception as well as the role of perception in learning" (39, p. 332). One may even suspect that Skinner (60) has made a concession of sorts when he writes of the "interpreted stimulus."

Aim of the present paper. Having assumed a metaphysical bias concerning the status of perception in psychology, I might follow any of several paths of discourse for the remainder of this paper. I have simply chosen to review critically some ideas about the nature of perception that have been prominent during the last fifty years, at the same time recognizing that thumbnail sketches of theoretical orientations involve grave risk of distortion and oversimplification.

AN OVERVIEW OF CONCEPTUAL DEVELOPMENT

I have titled these remarks "Perception: Evolution of a Concept." It may be countered that while change in conceptualization is evident, this does not constitute evolution, and that while there is increased complexity in the concept's definition, this is not the unidirectional unfolding which is characteristic of evolution—it would even appear that some of our notions have cycled full circle. Indeed, close examination of the recent history of thinking about perception prompts the suggestion that these changes might more properly be described as spiraling than as circling. For example, early definitions were empiricist only in the broad common-sense fashion that the metaphysics of Locke *et al.* was empiricist. In the early behaviorist's attempts to apply the systematization of science to psychology, the problem of perception was solved by eliminating the concept. Later systemizations (e.g., Hebb's) have been increasingly rigorous, and at the same

time have dealt with phenomena covered by the concept. Indeed, in Hebb's system, perceptual development has central status.

When physiologizing in psychology was in vogue 35 years ago, perception was rejected as a mental phenomenon. Today, physiologizing is again in vogue, but physiological systems attempt to accommodate the behavioral facts of perception. Finally, early in the period we are considering, the role of experience in perception was broadly defined and accepted as a self-evident metaphysical principle. Later, it was repudiated by the demonstrations of the Gestalt movement. More recently, the importance of past experience has been reaffirmed and the nature of the relationship more precisely defined, this time through methodical experimentation.

Conceptual evolution can be seen, I think, in numerous ways. First, there has been an increasing sophistication of perceptual theory qua theory. Fifty years ago, perception was essentially defined by a single a priori proposition. Today, in any of the several theoretical orientations extant there are attempts to represent it by a logically related set of propositions, connected through explicitly specified operations to the level of empirical generalization. Inspect, for example, the later hypothesis theory of Bruner (13), and Postman (56). Admittedly, perceptual theory has not achieved the status of a physical theory like the dynamical theory of gases. Yet the recent successes of adaptation level in quantitative prediction make for extreme optimism (34).

Secondly, the purpose of perceptual investigation has come to include greater concern for the elaboration of theory through empirical study. Earlier, perception was a term used in the description of the content of consciousness. It represented the compounding of sensations and images of past experience. Later the study of perception meant a catalogue of demonstrations and principles designed to explain, as Koffka put it in his *Principles* (46), why things appear as they do. Still more recently, the aim has been to construct a class of empirically based propositions that may be fruitfully used—usually along with other classes of proposition—in the prediction of a wide repertoire of behavioral activities.

Thirdly, there perhaps has been a more clearly conscious appreciation of the role and status of models and a more discriminating use of them. One need contrast only the psychological atomism of Titchener (62) with the current physiological atomism of Hebb (32) or the application of physical field theory by the Gestaltists in the 1920's with its present use in Köhler's work on visual figure currents (49, 50).

Perceptual models have, for the most part, been material models. Even in psychophysics and in adaptation-level theory where the explicit model is formal, one can find, I believe, an implicit material model. Early in our fifty-year period, models were essentially static-descriptive. In modern psychophysics (31), in Gibson's psychophysics of form perception (26), and in adaptation-level theory (34) the models are of these types. (It is not intended to imply that these are of a lower order than dynamic models, for, as Helson has pointed out, the trick in science is to express dynamisms mathematically and therefore statically.) But as perceptual theories became more functional—i.e., as interest is turned toward the relation between perception and other aspects of behavior—models became more frequently dynamic models: e.g., those of Woodworth (73), Tolman (64), Brunswik (17, 19), Ames and Cantril (e.g., 21), Bruner and Postman (e.g., 13), and Vernon (65). Finally, the early models were molecular. As the holistic

properties of perception were emphasized, and later the status of perception as behavior stressed, the use of molar models become prevalent (contrast the psychological atomism of Titchener [62] and the functionalistic theory of Brunswik [17]). As interest returns toward physiologizing and toward dimensional analysis in some areas of investigation and theory construction, molecular models are again coming into use—e.g., the cell-assembly theory of Hebb (32).

Fourth, the concept of perception has changed in complexity, and definition has shifted from essence to process. Early conceptualization centered around simple sensory qualities and the memory images which are vestiges of them. These elements were related in an uncomplicated fashion to a rather restricted number of physical stimulus dimensions. In more recent formulations, a larger number of variables are included, and the nature of the intervariable relationships is regarded as more complex. The relational character of perception is stressed, intermodality effects and the role of background described, and the contribution of nonsituational—i.e., motivational and learning—variables is emphasized. The view has changed from one of perception as a static picture of the external world to perception as the resultant of a complex repertoire of interacting processes.

Finally, one gains the impression of an increased variety of media used in current perceptual studies. While the majority of investigations still employ adult human subjects, animals and children appear to be used more frequently.

SOME PERCEPTUAL THEORIES IN OUTLINE

The following sketches are arranged into four broad categories, roughly chronological. This, however, is not to be viewed as a rigorous classification, for many of these views overlap in some fashion or other. The reader should expect a discussion of the place of perception in recent learning theory. However, we shall not deal formally with this matter, for learning theorists have done little more than recognize, albeit to different extents, the importance of taking cognizance of perceptual variables. Skinner (60) dismisses perception as conditional seeing, by drawing a distinction between discriminative behavior and response to discriminative behavior. Berlyne (8) proposes a place for perception in Hullian theory as an intervening stimulus-producing response (attention is its momentary effective reaction potential), and Wickens (71) regards it as dependent upon the differential reinforcement of certain magnitudes within the heterogeneous stimulus complex confronting the organism. For Tolman (64) perception is one of three types of intervening-variable systems, characterized by categorizing dimensions and intensitive dimensions (qualitative and quantitative orders). Thus, unlike others, he provides a formal distinction between perception and other mediating processes, but unfortunately he does not make clear how these formal distinctions relate to the level of empirical observation. Meanwhile, no theorist has yet reconciled the many facets of perceptual data within his model for learning.

I. Perception as Conscious Content

Titchener's views. Titchener, in his 1910 textbook (62), described two types of perception—pure and mixed. Pure perceptions are multidimensional sensations, combinations of simple sensations under the laws of attention. While the end result might appear to be unique and unanalyzable (e.g., the flavor of a peach), it is nothing more than the sum total of sensations evoked by various aspects of the stimulus object. Pure perceptions are rare, and could occur

only in a truly naive perceiver, for they involve the grouping of sensations under the laws of attention. Mixed perceptions are more common and also more complex, for they involve the addition of images of past experience. These latter originally may be kinesthetic images, but as the organism matures and his experience increases they derive from all modalities. Furthermore, perceptions differ from sensations in more than complexity; they have meaning. Meaning in turn derives from context, that is, from the setting in which sensations occur. It is thus clear that meaning depends on the contiguous occurrence of more than one sensation. Meaning is an inherent property of perception, for the context that accrues from a situation is always the psychological equivalent of its logical meaning. Meaning is carried by images as well as sensations and, indeed, these latter may degenerate into a kind of symbolic shorthand, largely unconscious.

In this view the primary concern is with the content of awareness. While perception involves what the Gibsons (29) have called enrichment, its literal aspect (the perception of the material properties of things) depends on sensory processes, and thus is an invariant and faithful representation of that part of the external world with which it is concerned. The aim of psychological investigation is the analysis of experience through introspection. This, in any final analysis, is an exercise in categorizing, for the method provides no means of empirical verification or check of internal consistency. The goal of psychology is met when these analytical descriptions are complete. The question of etiology and development as an empirical problem never really arises.

James' views. William James' (45) conception of perception, like that of Titchener, is an enrichment theory. Like that of Titchener, it is concerned with the content of awareness. Perception is the consciousness of particular materially present objects. Although sensations cannot be identified except in a logical sense, perceptions are represented as the combination of sensations and reproduced attributes. Sensations have both intensity and extensity, "thingness" depending on the coalescence of different sensations. But what we find here that we do not find in Titchener is emphasis upon the constructive role of past experience. Every perception is acquired, and, indeed, as it is found in the adult, represents a long course of development of order out of chaos. Furthermore, every perception is of definite and probable things. We perceive the thing—and this point is supported by extensive reference to illusions—"which would most usually have given us that sensation" (45, p. 316). In fact, habits of perceiving become so thoroughly established that new and alternative perceptions are more and more difficult to achieve. "Old-fogeyism," James contends, "is the inevitable terminus to which life sweeps us on" (45, p. 328).

There is much in James that we see highlighted in later interpretations. The long course of development represented in perception has been emphasized by Hebb (32). James' notions of probable perceptions and old-fogeyism are brilliant anticipations of Brunswik's probability learning (15, 18), Ames and Cantril's assumptions (e.g., 21), and Duncker's functional fixedness (24). But he does not take the further step that the modern functionalists take by regarding perception as resulting from an interaction between perceiver and perceived. Perceptions are learned, but it is not clear that this involves anything more than the passive compounding of sensations and memories of sensations, providing a kind of catalogue of pictures, each available for view on ap-

propriate occasion. This half-way view is perhaps an inevitable by-product of James' concern with content. Meanwhile, his discussion of illusions constitutes an armchair preview of the type of demonstration that Ames and his followers have popularized in the last decade.

The Gestalt orientation. The Berlin Gestaltists were among the earliest to recognize the importance of asking why objects appear as they do. Their answer was simple and direct. Taking their lead from Ernst Mach's contention that what we perceive are things (48), they reacted against the elementalism of the structuralist, arguing that sensations are logical abstractions and not psychological phenomena. Thus, if perceptions are not constructed from sensations, they must be immediately given in experience, our nervous system organizing them out of what Einstein has called the "rabble of the senses." There is assumed to be an isomorphism between brain events and experience, the structure of perception mirroring the structure of cortical organization. The Gestaltists' rejecting of the elementalism of the structuralists also means a rejection of their empiricism. Koffka (46) completes the discussion of almost every major problem of perception (e.g., figure and ground, phenomenal constancy) with a dialectical refutation of empiricism. This, however, does not mean denying the possible significance of past experience. Both Wertheimer (69) and Köhler (47) recognize it as a variable, but object to its indiscriminate application as a universal explanatory principle. While the phenomenological description of perceptions as wholes suggests a preoccupation with conscious content, the Gestalt theory is basically neurophysiological in orientation. While meaning is a complex phenomenon, depending, as Köhler has noted (48, p. 73), on personal biography, certain meaning (that of object qua object) is given in the perception, and thus by the brain dynamics. Indeed, the problem of meaning is probably what took the Gestalists away from peripherally biased structuralism to their theory of cortical organization. Curiously, their theory is still one of the enrichment type. The important transition in thought is that perceptions, as the psychological counterpart of brain dynamics, are constructs, or, more correctly, operational links between physiological constructs and the level of empirical data. Gestalt demonstrations are the means by which the functional properties of central mechanisms are known. This is most clearly illustrated in Köhler and Wallach's monograph (49) on figural aftereffects.

II. Perception and Modern Psychophysics

Graham's behavioristic psychophysics. Modern psychophysicists are, by and large, S-R psychologists. Beyond the assertion that their basic data are simple discriminations, they leave the metaphysical issues to someone else. With a few brilliant exceptions (e.g., Helson's work on adaptation level), their aim is an encyclopedia of quantitative relations between certain aspects of stimuli, defined as energy functions, and responses, similarly defined. As Graham points out (31), psychophysical functions are special cases of the general positivistic formula, $R = F\ (a \ldots z)$, where all variables are constant except stimulus dimension a. Special pains are taken to create situations which yield quantitative, easily interpreted results. There is no expectation that universal concepts can be generated.

Several general criticisms have been leveled at the psychophysicists. First, it has been contended that the tradition of sensory psychology from which they spring has blinded them to the impor-

tance of central variables in behavior in the setting of the psychophysics experiment. Second, this same background tends to result in an overly simple notion of the basic determinants of perception. Third, the restricted designs demanded by psychophysical methodology preclude identification of interactions among the various determinants of perception, and thus limit the possibility of fruitful theorizing that might stem from the data that complex designs yield. Lastly, conditions of observation are so artificialized in order to meet the aims of precise control that the data yielded have only limited applicability.

Such criticisms are basically statements of an opposing bias. Whether they are more than this waits on an empirical check of their validity. The psychophysicist must also, of course, answer them at the level of concrete data. Meanwhile, the kind of definition of basic elements that this sort of approach gives is a necessary first step in the construction of a general theory.

Helson's adaptation-level theory. Examination of Helson's work over the last 25 years shows it to be shaped by interests in the Gestalt definition of the problems of perception and in the psychophysicist's aim of quantification. His recently introduced concept of adaptation level reflects particularly this latter influence. This he presents as the basis for a general theory of perception (**34**, **35**). In outline, it may be regarded as the basis for a general theory of behavior.

Adaptation-level theory begins with the assumption that the perceptual response may be represented as a function of the difference between some present stimulus process and an internal standard derived from a pooling of the effects of previous stimulation. The status of this standard, expressed in terms of some quantifiable property of the external stimulus situation, depends both upon the characteristics of stimuli (e.g., frequency of presentation, magnitude, nearness in time, etc.) and of their background. It is given expression as a mathematical equation. Thus the model employed, unlike those employed in the majority of current perceptual theories, is formal rather than material, although it may be suggested that behind the formal model is the notion of the organism as a logarithmic computer. Since the model is mathematical, the work which it has inspired is necessarily static-descriptive in nature. But the concept is functional and thus evolutionary and dynamic in intent.

Adaptation-level theory has functioned as a precise predictive tool in a variety of situations to which it has been applied in the last 10 years. It has, for example, provided for the quantification of the shift in the neutral point with a change in standard in the classical psychophysical weight-lifting situation (**33**, **34**). It has successfully reconciled phenomena like color constancy, contrast, and conversion. It has made possible the quantification of concomitant changes in hue, brightness, and saturation (**38**). It provides a rational basis for the understanding of anchor effects, and may be applied fruitfully to the study of figural aftereffects (**44**). Recently, Helson has used it to scale the value of verbal terms with quantitative denotations, to demonstrate the effect of the context in which the terms are used upon these values (**37**), and to investigate the effects of personal and group factors upon the expression of attitudes (**36**).

Helson regards adaptation level to be the means for translating the notion of perceptual organization into objective and quantitative terms. In principle it does this. Successful application to the more complex situations that characterize everyday life is thus basically an

empirical matter of identifying the variables and their properties that are important for organization—that is, of delineating in greater detail the three broad categories of factors that he identifies as stimulus, background, and residual. We are at present particularly short of knowledge of the nonstimulus variables. We have yet, for example, adequately to establish the criteria for differentiating relevant from nonrelevant stimulus input. When this is accomplished, A.L. provides a means of predicting, within the class of relevant variables, what constitutes equivalent stimuli. Before the advent of A.L. theory this could be accomplished only a posteriori, in empirical terms (10). Certain of the parameters important to A.L. are not yet unequivocally defined. It is not always sufficiently clear, for instance, what is background and what is residual stimulation.

Adaptation level is one of several concepts currently directed toward the aims of general theory. Its assumption of pooling is also found in Brunswik's probability learning (17). If it is redefined as a probability function, it is seen to resemble strongly Postman and Bruner's concept of hypothesis (13), and it is a quantitative version of Cantril's assumptive world (21). It is hoped that the theory will be expanded so that it will consist not only of the A.L. formula, but also of an entire set of related mathematical expressions that will give more precise formal definition not only to the reference level, but to the range—with reference to both intensity and time—of relevant stimulus input, the complexity of relevant variables, the rigidity-plasticity of the judgmental system, the stability of the reference level, the fineness of articulation within the judgmental dimensions, and other parameters that constitute the perceptual frame of reference. Just how many equations would constitute the formal set would depend, of course, on the nature of the phenomena to which the theory was applied. Such a set of equations would be analogous in function to the set that relates work to force and distance, effort to distance and resistance, mechanical advantage to effort and resistance, and power to work and time in Newtonian mechanics, or to the set that relates pressure, volume, and temperature in the dynamical theory of gases.

Gibson's psychophysics of surface and edge. Gibson is concerned primarily with literal perception (27), that is, the perception of objects qua objects. Furthermore, his objects are the solid objects of the three-dimensional visual world (26). Awareness of these, he assumes, is immediately given by virtue of a kind of isomorphism between the spatial properties of the visual stimuli and the retinal events to which they give rise. Two-dimensionality is thus a learned refinement of three-dimensional perception. One sees in Gibson's theory, as in Helson's, evidence of two major influences: the quantitative aim of psychophysics and the relational approach of Gestalt psychology. He envisages quantitative description where the elements are surface and edge rather than points of color. For him stimulation is ordinal, that is, defined by gradient relationships within the retinal process.

There is an explicit nativism in Gibson's approach, for perception is regarded as reflecting directly the properties of the retinal correlate. But, unlike the Berlin Gestaltists, Gibson stresses the correspondence rather than the noncorrespondence between the external environment and our perception of it. Learning for him is specificity learning: the gradual awareness of greater detail, the enhanced capacity for discrimination (29). Through learning, perceptions acquire meaning. But perceptions

must be structured before meaning can be acquired, although some meanings may be embryonic in the same way that literal perception is. This enrichment of perception by meaning, however, is not, in the strict sense, perceptual learning.

It is curious that while Gibson's insistence on a psychophysics of object perception spells objective science, his distinction between the visual field and the visual world is pure phenomenology. His most valuable contributions are the notion of ordinal stimulation and his geometry of continuous surfaces. He has indeed made it clear that the retinal correlate is *not* a picture of the external world. Meanwhile, it may be contended that he has overestimated the veridicality of perception by his assumption of an identity between perception and judgment that is open to question (cf. his studies on size constancy [26]). Certainly his insistence that perceptual learning is specificity learning requires a distinction between perception and conception that cannot confidently be achieved beyond the verbal level. Finally, his theory is a logical construction that still awaits systematic experimental evaluation.

Attneave's application of the information theory model to form perception. The persistent need to quantify perceptions typical of everyday life has, in the last several years, served to motivate attempts to apply the information-theory model to these phenomena. Attneave has been a leader in this approach (5, 6). He suggests (5) that if perception is thought of as information handling, it can be immediately seen that much of the information provided is redundant. If the visual field were fractionated by a grid, redundancy would be found in connection with areas of homogeneous color, persistent direction or slope of contour, symmetry of design, or uniform gradient of texture—indeed, any homogeneity of pattern. The Gestalt notion of good figure thus refers to some high degree of internal redundancy. The perceptual mechanism operates to minimize redundancy, encoding input into more economical form. Hence, identifiable forms may be communicated by simply presenting contour points of maximal curvature.

This view, like Gibson's, assumes an isomorphism between the spatial properties of the external object and the organism's perception, the faithfulness with which the perception matches the object depending upon the efficiency with which the perceptual mechanism scans and encodes the input. The theory thus implies an intact, completely developed scanner, the character of the perception reflecting essentially the character of the input. As it currently stands, the model deals with redundancy associated with the geometrical properties of the speciously present stimulus. There is yet no extension to the problems of temporal redundancy and the effects of past experience. The learning mechanism, when provided, would appear to be necessarily of the specificity type. At the moment there appears to be no room for new perceptions of the sort considered by the enrichment theorists—notably those of the "new look" variety. This, then, is a theory of literal perception. It is not clear how the scanning mechanism will be instructed to respond when the properties of perception are not directly inherent in the physical object perceived.

III. Recent Physiological Models of Perception

Hebb's cell-assembly theory of perceptual development. Hebb has presented a neurological theory of perception in his *Organization of behavior* (32). While basically of the telephone switchboard type, its design provides the complexity and flexibility necessary

to account for the field properties of perception. Its major purpose, however, appears to be to provide a vehicle for explaining what Hebb regards to be the basic role of experience—particularly early experience—in structuring perceptions. Like Berlyne (8), he deals with the phenomena of attention in the same model. Like James (45), he sees even the simpler perceptions in structure going through a long course of development. Like Werner (68), he recognizes the role of motor processes in perception. His model is an elaboration of Lorente de Nó's concept of reciprocal circuits, to which has been added an assumption of axon growth with exercise. (The status of this last assumption has been questioned. Sholl [59, p. 91] cites research by Haddara which reports that while large measurable changes take place in the dendrites of cortical neurons of mice after birth, no difference could be detected between the neuronal development of normal animals and those in which the eyes had been enucleated at birth.) Spread of excitation in the developing nervous system is at first essentially random. With repeated receptor stimulation, however, neurons become organized into functional units called cell assemblies. With continual exercise the stability of these units is enhanced. As experience increases and is enriched, cell assemblies combine into more elaborate temporal organizations called phase-sequences.

With these notions, Hebb explains the configurational properties of perception, expectancy, perceptual learning, perceptual selectivity and perceptual generalization. In short, he accounts for both enrichment and specificity, as well as for the relationship between perceptual and other psychological functions. Hebb's theory is a brilliant intellectual exercise, imaginative and insightful. At present, however, it is difficult to feel that it constitutes any more satisfactory explanatory device than some of the models we have already described. Defining schemata in neurophysiological terms does not make them any more accessible or, for that matter, real. Granted that Hebb has elaborated the internal structure of his model in more specific detail than other theorists, and that he has described in terms of this model the nature of the several psychological functions, he still has to provide a key for transliteration from the terms of his model to the terms of empirical observation. The chemist has operations that enable him both to produce and to identify precise changes in molecular structure. He can talk, for example, of adding a double bond at a certain position in a particular molecule. Can the psychologist talk in the same way about adding a closed-chain circuit to a cell assembly? While it would be manifestly unfair to expect this sort of specificity so soon from Hebb, his theory implies it; and unless a means is clear by which it ultimately may be attained, his picture of central mechanisms, no matter how detailed, is no more than metaphorical.

Werner and Wapner's sensory-tonic field theory of perception. This theory is a recent variant of Gestalt psychology. It begins with a need to account for what its authors refer to as the projective nature of perception as well as its active, interactive characteristic. At the same time Werner and Wapner reject the possibility of a simple motor theory because they feel that such a view could not describe the mechanism that accomplishes sensory-motor interaction (67, 68). Instead, they propose a sensory-tonic theory. Tonus, as they use the term, refers not only to the distribution of muscular tension, but also to the level and pattern of visceral reactivity. Perception is a "total dynamic process" patterned not only by the nature of sensory input but also by the

tonic status of the perceiving organism. Sensory and tonic factors not only interact but are also dynamically equivalent.

Werner and Wapner support this last contention by citing evidence from the recent neurophysiological literature that a simple dichotomizing of CNS functions into sensory and motor processes is difficult if not impossible. From the psychological literature they review data that have been traditionally catalogued under the label, "Unity of the senses," and in this connection it should be noted that the tension effects they describe must be mediated through sensory feedback, either visceral or proprioceptive. Their own experimental work demonstrates that the perceived spatial position of simple figures, the autokinetic effect, and the perception of movement in static drawings can be influenced by the asymmetrical arousal of muscular tension (30, 51, 66). For example, the position of the vertical can be shifted left or right by electrically stimulating the neck muscles on the left or right side, respectively. Indeed, though they discuss visceral tensions, and suggest that their theory provides a means of accommodating the problem of motivation and emotion in perception (i.e., the projective nature of perception), their observations have, for the most part, been confined to the relation between tonus in the voluntary musculature and visual orientation. The notion of pooling of effects has already been seen in Helson's adaptation-level theory, and Helson has suggested (34) that his quantitative approach will facilitate an analysis of sensory interaction as well as an understanding of its central mechanism.

Perception in the ethological theory of instinctive behavior. The ethologists, a contemporary group of European zoologists, have elaborated a theory of instinctive behavior that includes some Gestaltist-influenced notions about the nature of perception (61).

The term "instinctive response" covers a varying complexity of animal activities. For example, the simple swimming movements of the eel and the elaborate mating ritual of the stickleback are both typed as instinctive behavior. The appearance of an instinctive response depends upon the activation of an "Innate Releasing Mechanism." The ease with which this mechanism is activated depends in turn upon motivation (physiologically, a concatenation of hormone actions), internal stimulation, and intrinsic nervous activity. Motivation, furthermore, is also susceptible to alteration by certain external conditions called *orienting stimuli.*

Actual arousal of the I.R.M. depends —except in conditions of intense motivation when vacuum activities may occur—upon the animal's being confronted by certain *sign stimuli.* The male stickleback in mating condition must perceive the swollen belly of the female before he executes the courting dance; the young herring gull must perceive the red patch on the mother's bill before giving the begging response; the young gallinaceous bird must perceive the long-necked silhouette of the mother before giving the gaping response. These stimuli vary in complexity. They also have Gestalt character, and cooperate according to what Tinbergen calls the "rule of heterogeneous summation" (61). Thus perceptions, the theory implies, are immediately given, and depend upon species, age, sex, motivational state, etc. Even in the instances where the ethologists recognize learning (they give greatest attention to Prägung, one-trial permanent learning that occurs during some critical period, usually early in the animal's life), this does not include perceptual learning, but rather presupposes, particularly in the case of Prägung, that perceptual

organization is immediately achieved. This instinct theory, like all instinct theory, requires the assumption that the effects of learning are confined to the specific area of experience in which learning occurs. Riess' (58) study of "instinctive" nest building, as well as the extensive literature on learning set, raises doubt concerning the validity of such an assumption.

IV. Modern Functionalist Views of Perception

McDougall's conception of perception. The reader may question the appropriateness of including a brief discussion of McDougall's view in this section. But McDougall was among the early protestants against a preoccupation with conscious content. To talk about contents, he insisted, is to lead to continual confusion. Perception, rather, is an active interpretative process of the mind (53). All human beings begin with a capacity for experiencing. After repeated practice in perceptual thinking, all become capable of increasingly refined and complex discrimination and, at the same time, of rich elaboration of the colligation of sensory signs confronting them. Perception is thus a form of conception and it is impossible, as Adams reiterates later (2), to make more than a verbal distinction between them. McDougall's discussion of cognitive dispositions implies a basic unity in the organism's behavior. But there is no suggestion of the dichotomy between specificity and enrichment in perceptual learning that the Gibsons have recently insisted upon (29).

Woodworth's view of perception as motivated response. In the Woodworth of 1921 (72), one notes some clear-cut differences from the conscious-content views described earlier. While some attention is directed toward conscious content, perception is seen primarily as a function performed by certain areas of the brain. It consists of neurological events that follow sensation and precede motor response. This neurologizing in itself did not constitute progress over its contemporary arm-chair analysis clothed in terms of consciousness. But progress did lie in the shift in emphasis from content to function. Perception is a response and not a picture, an active analyzing and interpreting of signals provided in sensory stimulation, a knowing of the facts. As such, it transcends present stimuli and is a necessary antecedent to overt motor response. As an interpretative response it is both combining and isolating in function, the culmination of a process of discovery of information about the world. It is susceptible to the regular laws of learning.

This position is made clearer in Woodworth's 1947 note on conditioning (73). What is reinforced, he contends, is the perceptual response—something on the receptor rather than the effector side. Certain views of the world get fixed as a result of the consequences they have for the organism's adjustment to its environment. Thus the common impression that perception is a passive affair derives from the fact that certain views provoke almost immediate reaction. But when a new perception is in the making, it is seen to be a slow trial-and-check affair motivated by a basic will to perceive.

Here, then, perception is not a picture of the world, but a motivated process, part of the organism's active search for adjustment in the world. Woodworth's view of perceptual learning resembles, in at least one aspect, that of James: the result of continued reinforcement must certainly be "old-fogeyism." The description of perception as an active process identifies Woodworth with the transactionalism of the functionalists of the 1950's. But in Woodworth, as in James, the notion of a reciprocal

relationship between perception and action is not explicit. While perception as an antecedent to motor response most certainly influences the latter's character, the alternative possibility is not made clear.

Brunswik's functionalism. Brunswik may be classed among the modern functionalists in perception. He has left us, however, no highly elaborated theoretical structure, for his preoccupation was with methodological issues. It was his conviction that the nature and magnitude of S-R correlations must be established before a theoretical superstructure can be safely put up. His functionalism is indicated at the level of theory construction by his concept of *ecological validity,* and at the level of methodological implementation by his notions of *representative design.* It is also seen in the correlational character of his work and in his interest in probability learning—particularly in his work on perceptual probability learning (18). Here it is clear that he meant to relate the very structure of perception to pattern of past experience. His work on distal focusing and the constancies (16) reveals a bias toward veridicality, but this he correctly sees as a matter of degree, not as one side of a theoretical issue. Brunswik's view is relational-organizational in character. His views on probability learning and his concern with the relation of perception to goals and aims resemble aspects of James and of Helmholtz. There is an obvious similarity between what takes place in probability learning and in the development of Bruner and Postman's families of hypotheses (13) and of Cantril's assumptive world (21), though Brunswik's notions are probably more precisely formulated. Finally, we may suggest that probability learning is a quantitative complement of adaptation-level.

The transactional approach of Ames and Cantril. The view evolved by Ames, Cantril, and their students (21, 42) contains elements similar in some respects to a number of the views previously described—e.g., James (45), Brunswik (17), Woodworth (73), Hebb (32), and Helson (34). Indebtedness to Whitehead (70) and to Dewey and Bentley (23) is expressly recognized.

Perception, according to the transactionists, is a process by which an individual person, with a particular outlook, attributes significance to events in his behavioral environment. These significances are established through the past attainment of goals. Put more simply, perception is part of the process of interpreting present stimulus cues from the bias of expectation. Expectations, or assumptions, are the consequents of past experience and action. As they are repeatedly confirmed or reinforced (and it is appropriate to speak of reinforcement, since the criteria of evaluation are provided by the purposes of the individual perceiver), corresponding facets of perception become more stable and harder to alter—although changes within specific situations may be dramatically effected by a single failure of confirmation.

While there is no detailed treatment of the generality of an assumption and its strength in a particular situation, one may expect that persistent failure in confirmation in some particular situation may result in weakening of the assumption for other situations. There is, this group points out, almost an infinity of physical arrangements (equivalent configurations) which will produce the same pattern on the retina (43). These are thus all seen alike, and what is seen is what one has been familiar with through the transaction of living. Rooms may be constructed in a variety of physical shapes. All will have the same perceived shape—the cubicle of everyday experience. This perception can change dramatically, however, when

the viewer checks his assumptions about shape by feeling about the room and finding it not to be of normal shape. Presumably, if failure of confirmation of the assumption of cubeness occurred frequently enough, our normal response to the equivalent configurations for rooms would also change. The totality of assumption, strong and weak, that the perceiver holds about his surroundings constitutes his assumptive world. The physical world must thus remain an inference. Perceiving, not a passive imprinting of a world picture, is one aspect of an active transaction between the striving organism and its environment. Perception, therefore, is necessarily the product of learning. It is not immediately given by the properties of the physical world and receptor organ. The demonstrations devised by the transactionists, when considered together, emphasize the lack of correspondence between the physical and phenomenal worlds: perception is nonveridical, but it is also stable. It is, of course, not exactly appropriate to speak of nonveridical perception, since we are not dealing with a noncorrespondence between the real world and one's impression of it, but rather with noncorrespondence between inferences about the world described in phenomenological and in physical terms.

The transaction theory has been criticized for ignoring the possible role of stimulus configuration in the structuring of perception (**27**). But neither the theorists nor their critics have created a crucial situation for establishing the primacy of either stimulus configuration or learning. It has also been contended that the transactional view fails to provide for perceptual learning of the specificity type. This may be countered by pointing out that strengthening of assumptions is a matter of differential reinforcement, and it is exactly with this technique that specificity is established (e.g., in the conditioned salivary response of the dog to tones). When strength of hypothesis is defined in terms of probability statements, we note the similarity of this view to that of Brunswik. When the probabilities are described as weighted in terms of relevance, the possibility of formulation of hypotheses in terms of norms, in the manner of Helson's adaptation-level, becomes apparent. Specificity is then definable in terms of variable error about this norm.

Bruner and Postman and the "new look" in perception. The late 1940's and early 1950's saw a long and controversial series of experiments directed toward testing the functionalist proposition that perception is an instrumental activity serving the needs and wants of the individual perceiver. Bruner and Postman were leaders in this movement. It was their conviction that we perceive what we need to perceive and what we are used to perceiving (**14**). Autochthonous factors were accepted as a class of perceptual determinants, and it was recognized that there are situations where the properties of the stimulus configuration leave nothing more to be determined in perception. But it was maintained that autism in perception typically is strong. For example, when it was argued that the "new look" effects are produced in the laboratory under conditions of extreme stimulus impoverishment, the reply was that the conditions of viewing in everyday life (e.g., lack of specific expectation, the momentary glance, etc.) are conditions of stimulus impoverishment. The motivated perceiver is alert to the presence of personally relevant objects and, although on certain occasions recognition of their presence may be withheld from consciousness, he recognizes them more readily, tends to overestimate their size, etc. Thus accentuation, fixation, vigilance, defense, resonance, and selectivity

were presented as explanatory principles to take their place along with closure, symmetry, proximity, similarity, and good continuation.

The earliest attacks on the "new look" approach were criticisms of technique. Failures at replication were described, and flaws in experimental design pointed out (54). But the theoretical formulation has also been attacked as vague and equivocal. Luchins (52) rejects the distinction between formalism and functionalism, marshaling a great mass of data that demonstrate perception to be stable and veridical, and insists that the importance of rational processes in perception has been neglected. Hochberg and Gleitman (40) object that the Bruner-Postman type of theorizing makes explanations possible simply by elaborating, *post hoc*, the list of motives presumed to underlie perception. Howie (41) claims that the theory does not enable one to determine when one functional principle gives way to another, and Prentice (57) suggests that to define perception in the way that Bruner and Postman do is not only to inflate it beyond its traditional meaning but to reduce it to meaninglessness.

Bruner and Postman have come to recognize their principles (perceptual defense, accentuation, etc.) as descriptive rather than explanatory (13, 56), and have proposed a more general expectancy-type theory. According to this later formulation, perception begins with expectancies or *hypotheses*. Each hypothesis represents the arousal of a complex of cognitive and motivational processes by an environmental clue. The present stimulus situation provides "information" to the expectant organism. This is evaluated with expectation as the criterion. If the initial hypothesis is confirmed in some critical degree, it is strengthened; if not, it is set aside for a new hypothesis. Hypothesis strength depends on frequency of past confirmation, the number of alternative hypotheses available, and certain cognitive, motivational and social consequences. Like Berlyne, Bruner and Postman attempt to account for the active, directive characteristic of perception by proposing a mechanism that unites its "pictorial" aspects with set. If one thinks of hypothesis strength in terms of relative frequency of reinforcement, one recognizes the striking similarity between this view and that of the transactionist. Meanwhile, while expectancy is the basic functional principle, it is not clear how it relates, within the framework of the theory, to other psychological processes like the mechanisms of specific motives and of the several cognitive processes, e.g., memory. Distinction between hypotheses and other psychological processes at the operational level in specific instances of observation appears to be momentarily extremely difficult. Brunswik (17), meanwhile, has made a definitional distinction between perception and thought in terms of uncertainty-geared and certainty-geared functions. Operational distinction was demonstrated by presenting a size-constancy task like, on the one hand, a problem in direct perceptional observation, and, on the other, an arithmetic-reasoning problem. In the first case, responses were compactly and fairly normally distributed, and indicated approximate size constancy. In the latter case, there were many on-the-dot correct responses, but also many extreme and bizarre errors indicating confusion and the shortcomings of logical extension from misinterpreted cues.

Vernon's application of the concept of schema. Vernon's theory of perception is a functional theory that stresses the stability and orderliness of the perceiver's conception of the world (65). She is critical of the recent trend toward studies of motivation and perception for ignoring the intelligent operation of the

perceiver's background of knowledge in structuring perceptions, and for overestimating the transient effects of needs. She begins by recognizing that the external world consists of orderly arrangements of objects, and that these give rise to a stimulation pattern that is ordered. Meanwhile, she rejects Gibson's asumption (26) that ordinal stimulation gives immediate rise to the finished perceptual product. It is here believed that perceptions are the end products of a slow process of construction through manipulation of and experimentation with the external world. While admitting that the slow perceptual development suggested by Piaget (e.g., **55**) may, to some extent, be attributable to maturation, she insists that specific experience is essential to the perceiver's conception of himself and the world. Through experience is evolved a system for integrating and interpreting the information provided in stimulus input. Motivation plays a major role in both the differentiation and elaboration of these classificatory criteria.

This theory is, of course, an application of Bartlett's concept of *schema* to the process of perceiving (**7**). Its resemblance to Bruner and Postman's cycle of hypothesis formation, hypothesis evaluation, and hypothesis reformulation is clear. Unlike Bruner and Postman's theory, however, it stresses the long-term orderliness and veridicality of perception. Long-term orderliness is also seen in the transactionists' notion of the assumptive world, but the insistence upon veridicality is missing. Furthermore, to make the notion of *schema* comparable to the concept of *assumptive world,* it would be necessary to postulate explicitly some superordinate schema that would integrate the various schematic structures (e.g., those having to do with temporal and spatial properties, those dealing with their significances, etc.) into a single functional system. The long time necessary for the elaboration of a schema suggests that its physiological counterpart might be Hebb's cell assembly.

While Vernon has taken pains to discuss the relation of schemata to both perception and motivation, she has not made quite clear the properties of schemata, defined either in terms of details of theoretical structure or operations which differentiate these details at the empirical level. How are schemata differentiated from perceptions, memories, motivations? How are some schemata distinguished from others? How are they functionally related? What are the functional properties of schemata—their rigidity, scope, complexity, etc.? Until questions like these can be answered, we have achieved only a way of looking at perceptual phenomena, not a theory of perception.

Allport's concept of structure. In his recent book (**4**), Floyd Allport evaluates some 13 perceptual theories and formalizes his own views. In all theories reviewed he finds two common limitations: ineffective handling of the problem of meaning, and unwarranted preoccupation with quantitative law. More basic than this latter, he contends, is another type of law, that of event-structure.

Structures are defined through a geometry of on-goings and their interrelations at event-points. Time is defined, not as a linear function, but as the duration of events that close themselves through a cycle of operations. On-goings are activities distributed both spatially and temporally; event-points are points of "contact" among "on-goings." Structures are not randomly organized patterns, but may be predicted in probability terms. Events, since they are points of contact, have no dimensions; they simply occur. Hence loci within a structure are event-regions of

space through time in which events may or may not occur.

Perceptions are dynamically operating structures. Perceptual aggregates resemble Hebb's cell-assembly-phase-sequence scheme (32), save that Allport appears to push the notion beyond the microscopic level of the material hypothetical construct to the submicroscopic level of the nonmaterial, purely intervening variable. Should the theorist be able to formalize structures in unambiguous denotable terms, he would be able to achieve a fruitful, integrated theory. This is an aim that the present writer does not see that Allport—despite compulsive elaborateness and sometimes excruciating logic—has attained. One comes away from reading Allport with the feeling that he has been concerned more with translating the concepts of competing perceptual theories into the idiom of his own viewpoint and less with the construction of a set of sentences that will provide for the explanation and prediction of a repertoire of specific perceptual phenomena. When, for example, the constancies are discussed, it is in terms of finding a basis for Brunswik's probabilistic weighting of cues in the density of meaning-cycles and in interstructurance. When phenomena are analyzed (e.g., thirst-quenching behavior), one is likely to feel that Allport has done little more again than translate the elements of the observational sequence into the nomenclature of his hoop (cycle) analogy. In short, his perceptual theory (and there is none distinct from general structure theory which is applicable to all natural events) is so abstract and is stated in such broad terms that it contributes no more than a general bias—granted that this may eventually prove quite fruitful. Certainly it appears to lack the articulation needed in a theory that is going to be intimately tied to the work-a-day activities of the laboratory.

THE LOGICAL STATUS OF CURRENT PERCEPTUAL THEORY

Early conceptualizations of perception are marked by a concern with conscious content; later views regard perception as a class of response, different from other classes in accessibility (it is inferred rather than observed) and in function (it is an intermediary rather than a consummatory response). Perception as intermediary response was first assumed to stand in a unilateral relation to consummatory acts; more recently the relationship has been described as interactive. Definitions have been framed in phenomenological, physiological, and psychophysiologically neutral terms. Perceptions have, on the one hand, been equated with simple discriminations, and, on the other, identified as events so complex as to defy ready replication. Early use of the term included reference to both the physical properties of an object and its significance; later, primary concern was with object qua object; currently interest has again turned to the meaningful object. Earlier studies dealt primarily with the encyclopedic task of analyzing and cataloging the properties of perception, generically defined; modern studies are most frequently directed toward schematizing the interplay of factors that give rise to some particular set of perceptual responses. While some current work stresses the autistic properties of perception, most of it emphasizes veridicality. Early theorists regarded learning to be of paramount importance in determining perceptual structure; the Gestalt theorists of the middle period de-emphasized the importance of learning, with their powerful demonstrations of the organizing factors; modern theorists have restored learning, particularly early learning, to a position of prominence. Except in modern psychophysics, attention has been currently di-

rected toward independent variables of a central rather than peripheral sort.

The adequacy of current data for theory construction. The adequacy of available perceptual data for the construction of a general theory of perception has been questioned, for one reason or another. A common criticism leveled against early work was that the criteria for identifying the perceptual response were basically individual and inside the observer. This actually meant that the criteria for identifying the data lacked precision, and that the status of the data with regard to the requirement of intersubjectivity was undetermined. Of course, all perceptions have in common the fact that they can be made public only as discriminations, and, unfortunately, the methodological reaction against introspection has resulted, in most cases, in a highly restricted definition of discrimination. Until recently, experimental designs have usually allowed for only a two-category response with all but one of the dimensions that might affect the response held constant. While the objectivity of the resulting data is beyond question, the value of such designs for the creation of the multidimensional interactive theories currently favored is unclear—such designs frequently lack representativeness (17). But before we can achieve data commensurate with such a view, perception must be dimensionalized. The structure of the underlying mechanism cannot be defined fully by studies that provide for only the identification of relevant variables, although this is a necessary first step. This is tantamount to saying that the psychophysical approach must be exploited, regardless of one's theoretical bias, before higher-order theories can be competently achieved.

Perception—a theoretical concept. It is not improbable that the long-time preoccupation with conscious content resulted from a failure to recognize the distinction between perceptual phenomena and perception as a theoretical concept. But as Tolman says (63, p. 111), "The fundamental essence of any hypothesis is not, it would seem, whether it be conscious or not but the fact that it is a condition in the organism which sets the latter, whether human or subhuman, for a certain systematic selectivity of behavior."

Logically, perception is a dispositional concept, and, as indicated by the occurrence of controversies like nativism versus empiricism, organization versus stimulus-compounding, and autism versus veridicality, a dispositional concept cannot be defined by *any* fixed totality of reduction sentences. It is always possible that more will be added as observational situations change. In the language of the experimenter, these reduction sentences constitute a class of operational definitions. Hence it further follows, as Garner, Hake and Eriksen (25), and Bridgman (12) before them, have pointed out, no concept can be given definitive definition by a single operation. Overlooking this fundamental requirement has led many investigators to the misimpression that operational validity is, in the final sense, simply a matter of arbitrary definition. Thus perception has been equated with simple discrimination. This view has yielded clear-cut experimental results, but under such restricted conditions that their generality is unclear. Further, it has meant an inability to differentiate between the properties of a perceptual mechanism which we infer to exist and those of receptor and effector systems. Thus distinction between sensation and perception is a priori, and made purely at the semantic level. (It is recognized that there are levels of communication—i.e., in an achievement-oriented psychology like Skinner's with the white rat—where the distinction be-

tween central and peripheral mechanism is not necessary.) Garner *et al.* (25) cite perceptual defense as a recently promulgated concept that is ambiguous because the experiments from which it was derived failed to achieve operational clarity. But lack of clarity probably results also from a lack of appreciation of the function that the concept is to perform. (See, for example, Gibson's paper, "What is Form?" [28], in which he describes 10 different concepts of form encompassed in three general categories of definition.) The remarks of the present paragraph are not intended to imply that perceptions should not be identified with discrimination for, as was pointed out earlier, discriminatory responses are the only means by which perceptions are made accessible as objective data. But the concept, as it has evolved from human studies, implies multidimensionality. Thus, it requires objective definition by a system of discriminations, as Boring suggests (11), made explicit through experimentation, if only such experiments are performed.

A general weakness of perceptual theories. Perception as a dispositional concept depends as much upon assumptions associated with the invented mechanism as upon the empirical data to which it is operationally linked.

Perhaps the most widespread methodological defect of perceptual theories (and this holds for psychological theories in general) is the absence of a set of statements that relate propositions about the model to reduction sentences (what Campbell picturesquely called the "dictionary" [20]). Even where the model (e.g., Hebb's cell-assembly model) is delineated in detail, these are more or less lacking. Perhaps a prominent exception is Köhler and Wallach's formulation of the satiation theory of figural aftereffects. Failure to relate data systematically to characteristics of the mechanism has meant an inability to relate various properties of the mechanism to each other. Thus, it is difficult to make a scientifically meaningful distinction between sensation and perception, and perception and conception, or to differentiate operationally between the processes of attention and those of perception. Unless such theoretically grounded, operationally specified distinctions can be made there is no justification for these terms in the formal language of psychology.

Prospectus. Now that charges have been drawn, what must be done to improve the status of perceptual theory? Armed with the equipment of modern logical analysis, one might attempt a tour de force, creating a highly articulated theoretical structure, cataloging and correlating reduction sentences, placing assumptions about the model in propositional form, and identifying the conditional reduction sentences necessary to convert propositions into statements about data. Such an exercise would probably not come off. Science, as Conant points out (22), is not made that way. Unlike a system of formal logic, it is not closed. Rather it is always in the status of becoming—of uncovering new data and incorporating new concepts.

It may be advantageous, however, to explore our dependent variables, both intervening and otherwise, as dimensions to be scaled. Only after this is achieved can the characteristics of our data be tied to the structure of our model thoroughly enough to provide high-level explanations. My personal preference at this stage of psychology's development is for formal constructs. These do not commit us to a physiology that may some day trap us. (As Adams [3] insists, a psychical system may be just as objective as a reflex arc or a cell assembly.) They can be conceived of, first, in simple terms with few di-

mensions, and expanded and made more complex as data are acquired, without risking the necessity of having some day to be totally abandoned in the face of some impressive piece of incompatible evidence. And finally, their properties are easily conceived of as dimensions to be scaled. The possible fruitfulness of such an approach is indicated by Helson's work on adaptation level.

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A SIMPLIFIED MODEL FOR STIMULUS DISCRIMINATION [1]

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It has frequently been noted during the early phases of discrimination training that response probability increases in the presence of negative instances, or S^{Δ}'s. That is, responding in the presence of such stimuli does not decrease monotonically in strength as is the case in simple extinction. Skinner has attributed the temporary increase in S^{Δ} response strength to induction (**14**). Induction is defined as a consequence of there being common stimulus properties between S^{D}'s and S^{Δ}'s.[2] The greater the similarity between the stimulus sets, the more induction takes place. The more induction present during discrimination training, the more difficult is the establishment of a sharp discrimination.

One of the major contributions of the Bush and Mosteller paper on stimulus generalization and discrimination (**2**) is the formalization of the properties of the discrimination situation. To recapitulate somewhat, they categorize subsets of stimulus elements as shown in Fig. 1, where S is the set of stimulus elements which constitute the occasion for reinforcement if a response occurs. S' is the set which is the occasion for nonreinforcement if the organism responds. I is the intercept of S and S'. It should be noted that I exists *always* in discrimination, whether or not the experimenter so contrives it. For example, the intercept includes all uncontrolled stimulation common to both S and S' stimulus sets, in addition to whatever the experimenter may or may not have contrived experimentally to be common to both sets.

Certain objections can be raised to the Bush-Mosteller model in regard to the number of constants to be estimated, and in particular to the statement that the measure of the intercept decreases as discrimination learning increases. Clearly the physical nature of the stimulus set is unchanged by the fact that an organism has come to make a discrimination. The present development owes much to the Bush-Mosteller formalization, but takes its roots in the independent though par-

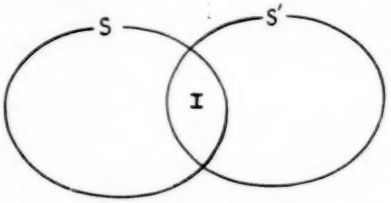

FIG. 1. Set diagram indicating overlap (I) between stimulus elements present on occasions for reinforcement (S) and elements present on occasions for nonreinforcement (S').

[1] The writer is greatly indebted to J. L. Snell and J. G. Kemeny of the Department of Mathematics at Dartmouth College for assistance and encouragement in the development of the model.

[2] Two conventions will be used throughout the remainder of this paper. In place of S^{D} and S^{Δ}, S and S' will be employed respectively to facilitate identification of parameters in this and the Bush-Mosteller paper. In addition, in the treatment of discrimination learning, the term *trial*, as employed by Estes *et al.*, is used in a broad sense to include the presentation of discriminative stimuli in a series. Rate of response has been shown to be directly related to probability of response as treated herein, and this usage makes evident certain possible extensions of the model to the free operant situation.

allel learning model of Estes and Burke (5). As in both the Bush-Mosteller and the Estes-Burke models, conditioning and extinction are conceived of simply as probability transfers between response classes. In this treatment the additional simplifying assumption is also made that behavior other than that under immediate consideration is mutually exclusive of that behavior. In simple conditioning the mean probability of response on the nth trial has been defined to be:

$$\bar{p}_n = 1 - (1 - \bar{p}_0)(1 - \theta)^n. \quad (1)$$

Symmetrically, in extinction, probability of response is:

$$\bar{p}_n = \bar{p}_0(1 - \theta)^n, \quad (2)$$

where $\bar{p}_0$ is the initial probability of response, or operant level; θ is the sampling ratio, or the probability of sampling a particular element; and n denotes the number of trials.

THE MODEL

Basically, one may conceive of discrimination as being the simultaneous or successive (depending upon whether stimuli are presented in pairs or serially) conditionings and extinctions of behavior appropriate to the sets of stimuli employed. The present treatment deals primarily with serial presentation, but can be adapted to the simultaneous procedure. It is appropriate to observe that Equation 1 describes the expected change in response probability associated with stimulus subset $S-I$, and that Equation 2 describes theoretical changes in behavior associated with $S'-I$. Were it not for the intercept elements, the present model would simply predict symmetrical changes in S and S', but the behavioral dynamics associated with I complicate the picture.

Estes and Straughan (6) developed a more generalized function to describe the results of sampling from the same stimulus set in both conditioning and extinction trials. The function is:

$$\bar{p}_n = \pi - (\pi - \bar{p}_0)(1 - \theta)^n, \quad (3)$$

where π represents the relative frequency of reinforced trials in the stimulus series. One may regard this function as representing a special case of discrimination, where probability of sampling a stimulus element is the same whether or not it is in S or in S', and where discrimination between S and S' is impossible. Within this framework, π is the asymptote which response probability approaches.

θ is defined as the probability of sampling a particular stimulus element. In the original paper by Estes (3), the sampling ratio was also defined as the ratio s/S, where s represented the number of elements sampled on a given occasion, and S represented the size of the population to be sampled. It is assumed that the *number* of elements sampled on a given occasion, s, remains constant. With θ so defined, it is seen to be determined both by organismic and by environmental factors. For example, should the organism sample exactly one element on any given trial, and should the population to be sampled consist of exactly one element, then $\theta = 1.00$. If, on the other hand, the population should consist of twenty elements, the value of θ would be 0.05. Thus θ is seen to depend upon environmental factors. Should the organism suffer impairment of its discriminatory apparatus, one might predict that the number of elements which it is capable of sampling on a given occasion is reduced. Thus the effect upon the value of the sampling ratio of impairing the discriminatory apparatus would be comparable to increasing the size of the stimulus population. Burke, Estes, and Hellyer (1) have shown that

rate of conditioning is inversely proportional to signal variability. Increasing signal variability, in other words, is one way to decrease the sampling ratio. Similar observations have been made in a more conventional discrimination experiment (**8**).

Once it is assumed that θ is a parameter determined both by organismic and by environmental factors, one may examine the consequences of assigning different values to θ under conditions of reinforcement and nonreinforcement. Let $\bar{\pi} = 1 - \pi$, θ_1 being the sampling ratio for S elements, and θ_2 the sampling ratio for S' elements.

Consider a single element in the intercept. One can form a Markov chain with States 0 and 1. State 0 means that the element is not conditioned, and 1 that it is (**11**, Chap. 7, Sec. 5). The transition matrix for this chain is:

$$P = \begin{array}{c} \\ 0 \\ 1 \end{array} \begin{array}{c} \begin{array}{cc} 0 & 1 \end{array} \\ \begin{bmatrix} 1 - \pi\theta_1 & \pi\theta_1 \\ \bar{\pi}\theta_2 & 1 - \bar{\pi}\theta_2 \end{bmatrix} \end{array}$$

The entry $p_{ij}^{(n)}$ of P^n gives the probability that the system is in State j after n steps if it starts in State i. But

$$P^n = \frac{1}{\pi\theta_1 + \bar{\pi}\theta_2} \times \begin{array}{c} \\ 0 \\ 1 \end{array} \begin{array}{c} \begin{array}{cc} 0 & 1 \end{array} \\ \begin{bmatrix} \bar{\pi}\theta_2 - \pi\theta_1\lambda^n, & \pi\theta_1 - \pi\theta_1\lambda^n \\ \bar{\pi}\theta_2 - \bar{\pi}\theta_2\lambda^n, & \pi\theta_1 + \bar{\pi}\theta_2\lambda^n \end{bmatrix} \end{array}$$

where $\lambda = (1 - \pi\theta_1 - \bar{\pi}\theta_2)$ is the principal eigenvector for P.

The mean response probability with a single stimulus element is $p_{01}^{(n)}$ if the element was initially in State 0, and $p_{11}^{(n)}$ if it was in State 1. Assume now that there are N stimulus elements in the intercept, a fraction, $\bar{p}_0$, of which are conditioned; i.e., in State 1. Then the mean probability of a response becomes:

$$\bar{p}_n = \bar{p}_0 \cdot p_{11}^{(n)} + (1 - \bar{p}_0) p_{01}^{(n)} \quad (4)$$

which is

$$\bar{p}_n = \frac{\pi\theta_1}{\pi\theta_1 + \bar{\pi}\theta_2} - \left[\frac{\pi\theta_1}{\pi\theta_1 + \bar{\pi}\theta_2} - \bar{p}_0\right] \times (1 - \pi\theta_1 - \bar{\pi}\theta_2)^n. \quad (5)$$

It can be seen that this last expression reduces to Equation 3 when $\theta_1 = \theta_2$. It follows that probability of response in S is a weighted mean of Equations 1 and 5 if we somehow take into consideration the fact that the intercept elements are presented within every stimulus presentation in the series, and $S-I$ and $S'-I$ are presented with frequency π and $\bar{\pi}$, respectively. By altering Equation 1 to

$$\bar{p}_n = 1 - (1 - \bar{p}_0)(1 - \theta_1)^{\pi n}, \quad (6)$$

and Equation 2 to

$$\bar{p}_n = \bar{p}_0(1 - \theta_2)^{\bar{\pi} n}, \quad (7)$$

one accomplishes the necessary correction. Now $\bar{p}_n$ refers to a response probability in the presence of the nth stimulus, regardless of whether or not it is in S or S'. Where π equals $\bar{\pi}$, I is operated upon twice as often as either $S-I$ or $S'-I$. If we define $\bar{p}_n(S-I)$ to be $\bar{p}_n$ of Equation 6, and let $\bar{p}_n(I)$ be $\bar{p}_n$ as in Equation 5, then

$$\bar{p}_n(S) = k\bar{p}_n(S-I) + (1-k)\bar{p}_n(I) \quad (8)$$

and, if we let $\bar{p}_n(S'-I)$ be $\bar{p}_n$ as in Equation 7, then

$$\bar{p}_n(S') = k'\bar{p}_n(S'-I) + (1-k')\bar{p}_n(I) \quad (9)$$

where k denotes the proportion of elements in $S-I$, k' the proportion of elements in $S'-I$, and $(1-k)$ and $(1-k')$ the appropriate proportions of elements in I for S and S', respectively.

PROPERTIES OF THE MODEL

The present model predicts the characteristic increase and decrease in S' response probabilities under certain special conditions. If $\bar{p}_0$ is greater than 0.00 and less than the asymptotic probability for intercept elements, and if the total number of elements within the intercept constitutes an appreciable proportion of the total in S', then the model generates the S' peak. The increase in S' response probability is produced when the conditioning to the intercept elements is more rapid than is the extinction to the $S'-I$ elements. Once the asymptote for intercept elements is approached closely enough and the rate of conditioning decreases, then extinction to $S'-I$ elements becomes evident, and S' response probability appears to reverse and decline to its terminal value. The model predicts that no initial increase would result if $\bar{p}_0$ were equal to or greater than $\frac{\pi\theta_1}{\pi\theta_1 + \bar{\pi}\theta_2}$, since $\bar{p}_n(S')$ would decrease monotonically. Likewise, if $\bar{p}_0$ were 0.00, $\bar{p}_n(S')$ would rise monotonically to some terminal value.

Since

$$\bar{p}_\infty(S') = (1 - k')\frac{\pi\theta_1}{\pi\theta_1 + \bar{\pi}\theta_2} = \frac{N_{(I)}\left(\frac{\pi\theta_1}{\pi\theta_1 + \bar{\pi}\theta_2}\right)}{N_{(I)} + N_{(S'-I)}}, \quad (10)$$

where $N_{(S'-I)}$ is the number of elements in $S'-I$ and $N_{(I)}$ is the number of elements in I, one can empirically alter the number of elements in $S-I$ and $S'-I$, and, by evaluating empirical asymptotes, determine the extent to which uncontrolled stimulation is weighted within I. Such a specification makes contact with information theory, since intercept elements constitute "noise" in discrimination.

The rapidity with which the increase in $\bar{p}_n(S')$ occurs, when it does occur, depends upon the slope parameters $(1-\theta_2)$ and $(1-\pi\theta_1-\bar{\pi}\theta_2)$. As shown in Fig. 2, the smaller the θ values, the more delayed will be the appearance of the peak within the series, and the greater will be the rise in the peak.

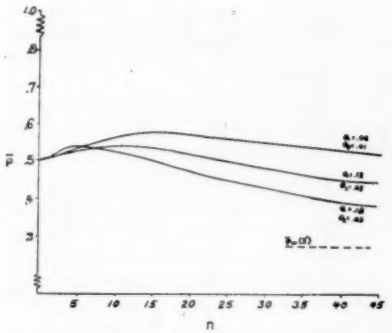

FIG. 2. Effects in appearance and magnitude of the S' peak as a function of the value of the slope constants.

One final comment is perhaps due. The term *stimulus induction* has been used synonymously with *stimulus generalization* (**9**). It seems reasonable to propose that induction be reserved in the present analysis to refer to the dynamics of successive conditioning and extinction within the intercept. This leaves generalization to the more static conditions treated in the Bush-Mosteller paper. Such a definition seems more in line with Skinner's original usage of the term.

DISCUSSION

Since θ values depend both upon organismic and environmental factors, manipulation of either of these has corresponding effects. Specifically, if the sampling ratio is reduced, either by increasing the effective stimulus field or by interfering surgically with the organism's discriminatory apparatus, the S' peak will be delayed in its appearance within the stimulus

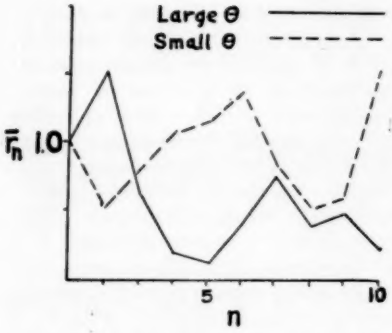

FIG. 3. Displacement of empirical S' peaks resulting from reducing the total number of stimulus elements to be discriminated.

series. On the organismic side, Dr. Karl H. Pribram, after inducing temporal lesions in rhesus monkeys, reports both the expected delay in the appearance of the S' peak and the expected differential in magnitude of the peak in the behavior of operated animals as compared with normals.[3] Data from previous studies (**7, 8**) support predictions of the model concerning the appearance of the peak as a function of manipulations of the stimulus field. Figure 3 shows the effect upon the position of the peak of increasing the sampling ratios. The increased sampling ratio was produced by reducing the total number of elements to be sampled in S'; the effect is an earlier appearance of the peak.

An experiment was performed explicitly to evaluate the reality of the model's predictions with regard to presence and absence of the peak.[4] Ten subjects were run in each of two groups. In each group an equal number of cards with S and S' patterns were shown the subjects. The subjects were required to state when they believed an S pattern was being presented; they were informed when they were correct. The series of patterns was shuffled between experimental runs, so that each subject was exposed to a different series of patterns. The patterns were composed upon cards upon which were drawn ten circles in two lines of five circles each. In Group I, one particular circle was always filled in if the pattern was an S. A different circle was filled in to define S' instances. Three other circles, comprising the intercept, were filled in in various combinations of one, two, or all three; and these were presented equally often with either S or S' cards. In Group II, the S and intercept circles were filled in, as in Group I, but S' of this latter group consisted of the remaining six circles, one of which was always filled in. In brief, the technique is the same as has been used in a free operant situation.[5]

Under both conditions, the subject is faced with a guessing problem at the outset, hence an estimate of .5 for $\bar{p}_0$ seems not unreasonable. The model makes explicit determinations of several constants for these experimental conditions, as is shown in Table 1.

Consequently it remained to estimate but a single parameter; either θ_1 or θ_2 in the Group II condition, since both θ_1 and θ_2 of Group I are equal to

TABLE 1

DETERMINATIONS OF THEORETICAL CONSTANTS

Group	Constant					
	θ_1	θ_2	$\bar{p}_0$	k	k'	$\frac{\pi\theta_1}{\pi\theta_1 + \bar{\pi}\theta_2}$
I	.18	.18	.50	.25	.25	.50
II	.18	.03*	.50	.25	.67	.86

* Estimated empirically.

[3] As reported in his unpublished manuscript entitled, "On the neurology of thinking."

[4] Acknowledgment is due E. P. Stichman who conducted experimental work in the early phases of this study.

[5] A fuller report of this experiment is being prepared by the writer.

θ_1 of Group II; and since all of these are six times as great as θ_2 of Group II, θ_2 of Group II was estimated to be .03. Two principal points were examined in this experiment: one being the prediction that the equal θ case should generate S and S' curves which were symmetrical about $\bar{p}_0$; S and S' curves in the unequal θ case should, however, be characterized by a more rapid acceleration to S asymptote than is true in the equal θ case, and an S' peak should occur which should be followed by a slow decrease to S' asymptote. Figure 4 demonstrates the gross relation obtained between theoretical curves and the data gathered in this experiment.

The second prediction was that, aside from the gross characteristics of the curves, reasonably accurate predictions concerning exact response probabilities should be rigorously made by the model. The theoretical distribution for the curves is not known, so a χ^2 test of goodness of fit was performed. In Group I the levels of confidence for S and S' were $(.6 < p < .7)$ and $(.7 < p < .8)$, respectively. In Group II they were $(.9 < p < 1.00)$ for both curves. Admittedly the fit is best for the condition from which the estimate of θ_2 was taken, but fitting all four curves through the estimation of one parameter with the degree of precision that was attained in this experiment is encouraging. Certainly the model is distinguished from others in the uniqueness of its predictions concerning a given experiment.

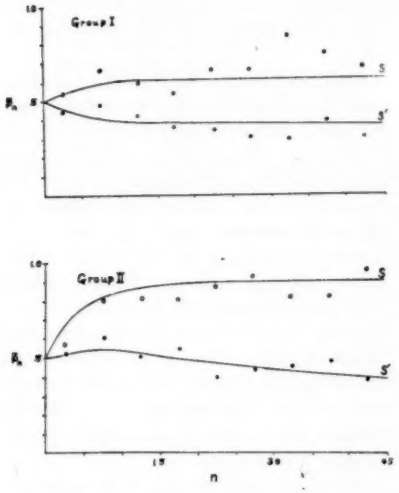

FIG. 4. Theoretical curves and experimental results where $\theta_1 = \theta_2$, Group I, and where $\theta_1 = 6\theta_2$, Group II.

In examining what happens in the experimental setting, one observes activity reminiscent of Kantor's analysis of a response within a behavior segment (**10**). Kantor has defined a response as being composed of three reaction systems: the attending, the perceiving, and the consummatory. Explicitly, in this and similar experiments, the subject is required to orient toward, or attend to, the stimulus patterns, to respond visually, and to emit some operant response. In the model, both conditioning and extinction are regarded as probability transfers between an observed class of behavior and some mutually exclusive classes of behavior, without examining the nature of the non-observed classes. As conditioning takes place, competing behavior decreases in strength. As extinction occurs, competing behavior takes the place of the experimentally measured class. The kernel of the argument is that during extinction, as competing behavior increases in probability, the competing behavior carries with it its own postural or orientational aspects. These changes in orientation, or physical position of the subject, systematically increase the relative size of the $S'-I$ set, thereby reducing the probability of sampling an S' element. This has the effect of

distorting estimates of both the sampling ratio and the constants of proportionality.

It logically follows that a corresponding distortion should occur in S on conditioning trials. The value of θ should increase as conditioning progresses. Estes has already indicated that this is the case (**4**). Hence the more rapidly conditioning progresses, the more rapidly will it progress. This may have some relevance to analyses of "learning sets." At the same time, if extinction is rapid at first, it may be expected to interfere with itself, so to speak, since sampling efficiency is reduced.

The second distortion so introduced is in the constants of proportionality, k and k'. Initially, uncontrolled stimuli are present as a part of the intercept. As the systematic changes take place, as described above, which translate much of this uncontrolled material into $S'-I$, I is reduced relative to both S and S'. It is here that some behavioral meaning can be given the Bush-Mosteller statement about the diminishing intercept. Such a displacement tends to exaggerate the departure of the S and S' curves from $\bar{p}_0$; thus an analysis is possible which accounts for the occasional instance of near-perfect discrimination in a manner which is preferable to some others which have been invoked to account for this phenomenon elsewhere. The discrimination operator D in the Bush-Mosteller model is clearly a mathematical device introduced with little rationale other than the fact that it produces curves which look more like discrimination curves. Restle's adaptation model is also designed to handle this problem, but it unfortunately introduces more elaborate assumptions concerning the conditioning process (**12, 13**). The present analysis would seem preferable since it involves no more complex assumptions concerning the conditioning process than does the original Estes-Burke theory, and since exact experimental tests of its correctness are immediately evident.

If orientational changes do in fact distort the parameters k and k' as a function of trials, they should do so in a given experimental setting to a greater or lesser degree, depending upon the number of experimental elements actually manipulated within the three subsets. That is, the fewer the number of experimental elements, the greater is the relative contribution of the uncontrolled stimulation and the greater the distortion produced in the constants of proportionality when the uncontrolled elements shift. The more elements existing within the sets, the less is the relative contribution of uncontrolled stimulation, and the less will a shift distort k and k'. In the experiment presented above, the greatest distortion does indeed exist where the fewest elements are involved. While this is only suggestive that the present analysis is correct, current experimental work is under way to evaluate this phase of the problem in detail.

One technique to reduce the systematic distortion has been proposed by Estes, where intertrial intervals are increased. One might reason that this provides ample opportunity for the subject to sample everything in the environment, such that any systematic sampling in S' would exert little relative effect. In regarding orientational changes as stimulus producing, this reduces to the analysis above in that the total number of stimuli is increased. Certainly a mapping of the response space is a sorely needed development in further enterprises of this kind.

The alternative to this analysis would seem to involve the introduction of additional complicating assumptions; while any number of

mathematical devices can readily be assumed which more or less solve the curve-fitting problem, the experimental approach remains the more satisfying.

SUMMARY

A particular examination of the S' peak from the standpoint of an extension of the Estes-Burke statistical learning theory supposes the peak to be generated by simultaneous additive and subtractive processes working upon response probabilities associated with stimulus elements within and outside the intercept of the S and S' subsets. Certain exact experimental predictions are made and examined, and some questions concerning its use in developing a program of research are raised as it compares with other current approaches. In the light of available evidence, extensions are suggested for broadening the scope of the present analysis.

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